SCIENCE

29 November 1957

Volume 126, Number 3283

ERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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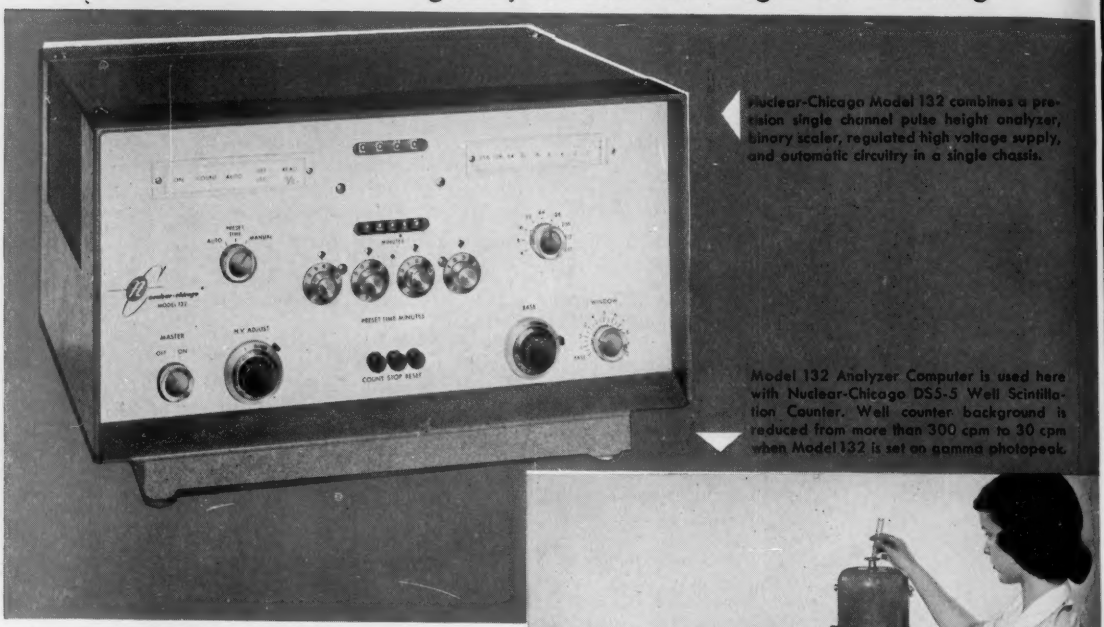

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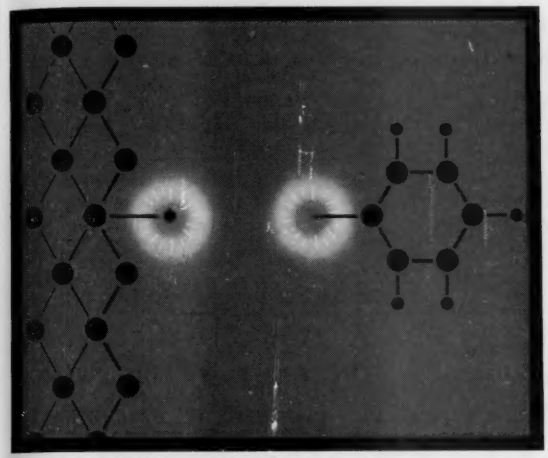
9



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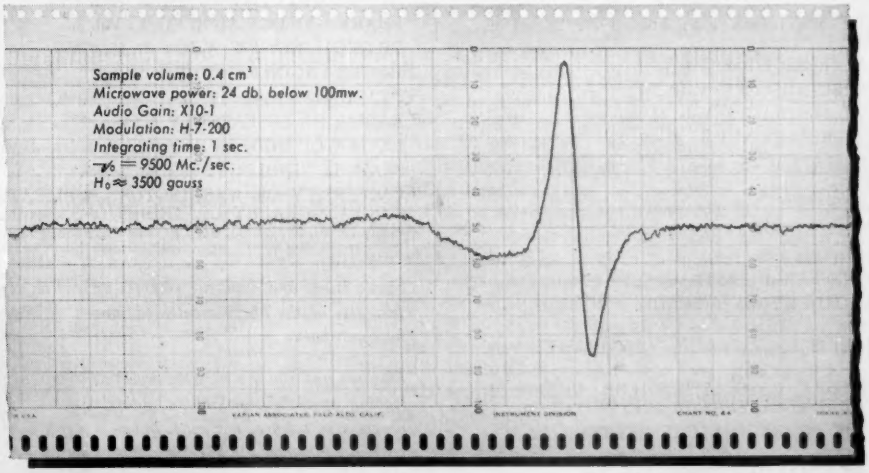
This spectrum has been furnished by courtesy of Dr. H. E. Beinert of the Institute for Enzyme Research, University of Wisconsin.



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SCIENCE

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Wanted: Consistency

Current emphasis on science in the thinking of Congressmen and other public leaders illustrates both a good trend and a bad habit. To have greater attention given to the welfare of science is good, but to have attitudes change so quickly and radically is a part of the inconsistent, on-and-off support that interferes seriously with steady scientific progress. Although public education rather than scientific progress itself is involved, the history of support for the international exhibition to be held in Brussels next year illustrates the point. If the budget for American scientific exhibits were up for vote now, liberal support would be almost certain. In 1956, adequate support was planned and partly appropriated. Last spring, Congress cut the total appropriation by about 25 percent.

On the basis of the original budget allotted by the State Department to the National Science Foundation for science exhibits, U.S. planners promised the other participating nations a specific list of exhibits to be included in an integrated, international portrayal of the story of scientific progress, a story that will be presented in four sections—on the atom, the molecule, the crystal, and the living cell. When the cut came, all of it had to be taken from exhibit funds, for the cost of a building (not for science, but for other U.S. exhibits) could not be reduced, and neither could a number of other fixed charges, such as that of staffing the displays in Brussels. All phases of U.S. participation suffered, science included. Instead of an expected $600,000 for the actual construction of scientific exhibits, only $280,000 was available.

U.S. planners faced a hard choice: Should they cheapen exhibits or eliminate some? They did both. Some vivid operating exhibits were replaced by static panels or wall charts. Others were canceled entirely.

This is not to say that science in the U.S. will be poorly represented. Far from it. The exhibits have been planned with great care by a committee under the chairmanship of Paul Weiss, with Ernest Lawrence taking primary responsibility for the atom, Henry Eyring for the molecule, Cyril Smith and Frederick Seitz for the crystal, and Paul Weiss for the living cell. These men and their committees have made the exhibits just as good as the limited budget permits. American industry has chipped in with $600,000 worth of donated equipment and time. Scientists and scientific industry have produced exhibits that will be good, but not good enough when comparisons between the U.S. and the U.S.S.R. are inevitable and when the U.S.S.R. seems to have unlimited funds with which to advertise its scientific achievements.

There are still some salvage possibilities. For $25,000 the science exhibits could be supplemented with a two-hour program of top-quality science motion pictures. For $60,000 the wall charts could be replaced by the originally planned operating models. Even these small sums are not available. Unless a generous angel appears quickly, there can be no improvement over present plans.

As for Brussels, we are reduced to hoping that the great international jury of visitors will be impressed by a handsome building, and that they like wall charts. As for the U.S., we hope that there are beginning to be enough examples of the essentially uneconomical nature of short-term, on-and-off financing of science so that we can look forward to longer term and more consistent support. Both science and the country will profit from such a change.—D.W.

The place of the Particle Accelerator in Basic Research...

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Denis M. Robinson

Denis M. Robinson, President

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29 November 1957, Volume 126, Number 3283 SCIENCE

Science in the U.S.S.R.

Introductory Remarks

The Iron Curtain surrounding Russian progress in earth science has recently been opened in a number of ways. One rift in the "curtain" has been an increasing participation by Russian scientists in international conferences. Formerly, these representatives were closely watched by special agents. Recently, however, individual scientists have mingled freely and privately with nationals of other countries, including our own. Another important development has been a freer exchange of scientists between this country and Russia, so that experts in virtually all branches of the sciences have recently visited major facilities behind the "curtain." In return, Soviet scientists have visited here and have been relatively open in exchanging information.

Perhaps the most important development is the translations of Russian journals, which are being made on an increasingly large scale. These translations permit detailed and thoughtful evaluation of the quality of Russian scientific work. The quality of published literature is one of our best indices of the magnitude of their effort.

Inorganic Chemistry

My remarks on the quality and quantity of current chemical research in Russia are based on translations and abstracts of the chemical literature that Soviet scientists have published in the past few years. The Soviet scientists have published much good work, and it is my loss that I am not able to read the Russian language. The domain of chemistry is so broad that in order to get to specific examples and to stay within the space allotted, I shall limit my discussion, with one exception, to the field of inorganic chemistry. That exception is in the field of physical chemistry and constitutes undeniable proof that the quality of Soviet research in this fundamental field is excellent. I am referring, of course, to the award of the 1956 Nobel Prize in chemistry—the world's top award in that field of study—to Sir Cyril Hinshelwood of England and Nikolai Semenov of Russia for their brilliant and independent research on chemical kinetics, which is a study of the manner in which molecules behave in a chemical process.

Inorganic chemistry is the oldest branch of chemistry, but, until recently, it suffered from lack of interest because the newer fields of organic, physical, and biochemistry that developed during the last century and the early part of this one offered so many more opportunities for original research.

The new technologies such as nuclear power, rocket and jet propulsion, and electronics have focused new attention on inorganic chemistry, and progress on many fronts in these newer technologies is dependent on progress in inorganic chemistry. As a consequence, both in the Western world and in Russia, there has been a marked increase in the amount of research in this field. One consequence of this increase is the publication of a new journal, *The Journal of Inorganic and Nuclear Chemistry*, to which the principal contributors are British, American, and French chemists. In its three years of publication, this Western journal has averaged 800 pages of research publication per year. Last year, a Russian journal of inorganic chemistry began publication and published 2800 pages of material. During the first six months of the current year, this Russian journal has published a little over 1400 pages of research material. The Western journal is still publishing at the rate of 800 pages per year. This indicates a ratio of about three and one-half pages of Russian publication for each one in the Western publication. This ratio, of course, is not an accurate index of the relative amount of inorganic chemical research in the two areas, because there are general journals that include inorganic chemistry and because the classification of a given paper as "inorganic" may be somewhat arbitrary in some instances, but it is certainly strongly indicative of the relative amount of such research. Contrary to the situation with respect to much Russian scientific publication of ten years ago, when much of the published material consisted of review articles of results published in Western journals or repetition of work previously done, the new Russian scientific publication is rich in original work, much of it of very high quality, and it covers a wide variety of topics.

Travel at supersonic speeds requires materials that will withstand extremely high temperatures without burning and without loss of strength. Current knowledge indicates that the metals tantalum, niobium, molybdenum, and titanium, either in super alloys or in combination with certain ceramic materials, are the most likely to withstand these extreme conditions. A detailed study of the chemistry of these elements is in progress in Russia as well as in the Western world. This ranges from a study of the geochemistry of these elements (which will ultimately pay off in improved prospecting methods) to detailed studies of the reactions of these metals and of their compounds. The latter studies will ultimately pay off in improved extraction and purification procedures. The Russians, too, are well aware of the extremely great increase in strength that is characteristic of metals of extremely high purity.

The development and wide-spread use of transistors in the field of electronics is well known. The chemical elements germanium and silicon, from which transistors are made, present problems which have to do with source of raw materials for the former and methods of purification for both. The Russians have given considerable attention to the chemistry

The six articles in this group are adapted from the six addresses that were presented before the 427th meeting of the Washington Academy of Sciences on 29 October at the Cosmos Club, Washington, D.C. The general theme of the program was "Science in the U.S.S.R." The introductory remarks were prepared by Philip H. Abelson, director of the Geophysical Laboratory, Carnegie Institution of Washington, Washington, D.C.

of these elements in an effort to meet these problems.

Certain of the newer compounds of boron have achieved importance in recent years because of their usefulness as jet fuels. Certain other new compounds of boron have become important as reducing agents in chemical reactions, and still others have been shown to have excellent potentialities as temperature- and abrasion-resistant materials. There is a fair amount of Russian literature concerning the two latter classes of boron compounds but little about the first class. This, I think, is due to security classification rather than to lack of interest.

The rare earth elements—a long neglected group—have achieved considerable prominence since World War II because they constitute a considerable portion of the residue from nuclear fission processes and because a number of them have interesting nuclear characteristics. The recent Russian literature shows that a considerable amount of up-to-date research on rare earth compounds is being carried out, but there is no indication that the rare earth metals are being extensively investigated.

The limitations of polymers made from organic materials have long been recognized. There is good theoretical basis for believing that some of these limitations could be overcome if controlled polymerization of inorganic materials could be achieved. Much effort in this direction has been expended, but with only a limited amount of success. Polymerized silicate ions have been obtained, of course, but they are unsatisfactory in many respects. Soviet scientists have recently reported that they have succeeded in incorporating other metals into the silicon-oxygen-silicon chain. How far this line of research will go and how successful it will be is yet to be determined.

I have avoided reference to the inorganic chemistry of fissionable elements deliberately rather than through oversight, because the Soviets' demonstrated success in this field has been obvious for some time.

Basic or fundamental research in theoretical inorganic chemistry has not been neglected. Studies in structure, stability, coordination compounds, oxidation-reduction phenomena, and so forth, have all received their share of attention in the land of Mendelejeff—one of the intellectual giants of inorganic chemistry.

In closing I should like to say that there is no great cause for concern that the quality of Russian-sponsored research in inorganic chemistry is superior to that of the Western world, but there is cause for concern in regard to the quantity of Russian-sponsored research in this field.

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Low-Temperature Physics

Low-temperature physics cuts right across any subdivision we care to make of the field of physics, and one might wonder, therefore, why it is that there is a separate group who call themselves low-temperature physicists. The reason is historical and has to do with the fact that the production of very low temperatures was, until recently, a specialized technique, and that there were few institutions that could afford the effort or expense of a low-temperature installation. This is not true nowadays, however, and in fact most laboratories in the United States, both university and industrial, have access to liquid helium. This has been made possible mainly through the development, by Collins, just after World War II, of a convenient type of liquefier, through its commercial manufacture, and through the considerable sponsorship of work in low-temperature physics by the Department of Defense. This has meant that the volume of work in this field in the United States has risen tremendously since the war; in fact, one refers in a friendly way to previous work in the field as being "B.C."—meaning, of course, "before Collins." I mention this because it is important to realize what an enormous growth has gone on in this country in recent years, apparently greater than the growth in Russia.

In Russia, work in low-temperature physics began in the early 1930's. For example, at Kharkov there were, on the experimental side, L. and B. Ruhemann, who had worked with Simon in Berlin, and, on the theoretical side, L. D. Landau. Incidentally, we might anticipate a little here and note how many times the name of Landau, who now works in Moscow, appears in Russian work. This is less true of Kapitza, who first published from Russia in 1939, and who in fact wrote the first two articles in the then new *Journal of Physics of the U.S.S.R.* These were technical papers having to do with the construction of liquefiers.

With regard to the physics of low temperatures, perhaps the most striking and typical phenomena are the transformation of liquid helium into a superfluid phase and the transformation of many metals and alloys into a superconductive phase. The Russians have been working steadily in these two fields since the early 1930's. They have made their most striking contribution in the field of liquid helium, through work triggered by Landau and Kapitza.

Liquid helium is a strange substance, and some of its properties have been understood more easily and for a longer period of time than others. It remains a liquid down to absolute zero and does not solidify at all at normal pressure: yet it undergoes a phase transition at 2.2°K below which it is a superfluid. This startling property of superfluidity and all its ramifications received an explanation only in terms of a model. Such a model was developed outside Russia by Tisza in 1938 and in a different way by Landau in 1941. Landau claimed that his model followed from quantum mechanics, and although there seems to be a universal opinion that he never rigorously proved this, the model nevertheless has had great success. One of the most notable things was the prediction of a new kind of wave motion in liquid helium, called "second sound." It should be said that it appears that Tisza realized more clearly at the time that "second sound" is a thermal wave. It was only after an experimental failure in Russia to excite "second sound" mechanically, by Peshkov, that it was suggested that a thermal device would be much more efficient; Peshkov was then successful and was, in 1944, actually the first to excite "second sound." The key experiment in the attempt to decide between the two models, however, was one designed to measure the velocity of second sound below 1°K; it came out convincingly for Landau. Many other types of experiments which are well explained by the Landau model have also been performed with liquid helium.

There seems to have been no further attempt in Russia to put the theory on a more rigorous basis. In contrast, there has been further work in this country, notably by Feynman and others.

In superconductivity one should mention the theoretical work of Landau on the so-called intermediate state and the beautiful experiments of Shalnikov and Meshkovsky which followed and which were designed to test the theory. The subject deals with the destruction of superconductivity by a magnetic field and the splitting up of a superconductor into superconducting and nonsuperconducting domains. It is a theory of a macroscopic effect—that is, one involving a very large number of atoms.

As far as the microscopic, or fundamental theory of superconductivity is concerned, the problem does not seem to have been tackled by the Russians. This field is most closely linked at present with the names of Fröhlich and Bardeen and their co-workers.

There are many relevant fields in which the Russians have made very little effort—for example, in magnetism and very low temperatures, especially electron spin resonance which was discovered independently by Zavoisky in Russia in 1946 and by Halliday in this country. This has been a very fruitful field of research but does not appear to have been exploited very much by them. It has been at Chicago and particularly at Oxford where most of the development

in this field has gone on. From it has come impetus for the growth of the new and interesting field of nuclear orientation, and experiments in this field include the recent one on parity nonconservation in β-decay. Then, too, I must mention the interesting and important technical device known as the solid-state Maser, which has also grown out of studies of paramagnetic resonance. The Russians do not appear to be publishing in this field.

The Russians have done very little work in the field of adiabatic demagnetization and the region of temperature below 1°K, although they seem to be exploiting the use of the rare isotope He^3 in this connection. On the whole, therefore, it seems that they have limited themselves to specific fields, particularly to the fields of liquid helium and superconductivity. It should be noted, however, that from the occasional theoretical paper which appears in a field outside the ones mentioned, one concludes that the Russians are very much aware of, and understand, the work in all fields.

Finally, I should like to say that physicists who have visited their laboratories are quite impressed, on the one hand by the energetic theoretical discussions which go on, and on the other, by the outstanding demonstrating and teaching equipment.

ERNEST AMBLER
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Nuclear Physics

My impression of the state of nuclear physics in the Soviet Union has been gathered entirely from the published literature available in Western libraries. I have never personally visited Soviet scientific establishments, but my reading knowledge of the Russian language has made it somewhat easier for me to obtain an over-all view of Russian scientific progress in this field.

A natural distinction imposes itself between theory and experiment in nuclear physics. In the former, Russian physicists, even before World War II, have always made important contributions comparable to those originating in other countries. Several excellent schools of theoretical physics exist in the Soviet Union. In all respects they are equivalent in quality of output to similar Western schools and, in fact, they show a considerable degree of originality and independence of thought.

In the field of experimental nuclear physics, on the other hand, quite a different situation is found, which, as we shall see, strongly indicates a special concerted effort along certain lines to equal and possibly top Western achievements. I shall omit from consideration the field of nuclear reactor physics and technology, where we have known, since the Geneva International Conference in 1955, that little if any difference now exists between the Russian and Western states of the art. In the high-energy (accelerator) field, virtually nothing was done in Russia prior to World War II. Only about four years ago the Russians suddenly and dramatically lifted a curtain of silence from an impressive array of machinery as well as from a three-year backlog of competent results obtained with it. A large number of specialists on linear accelerators, synchrocyclotrons, and synchrotrons (one of the inventors of the synchrotron is a Russian) are active in several institutes. The largest accelerator functioning in the world at present—a 36,000-ton giant yielding protons of about 10 billion electron volts—is located near Moscow.

A great deal of originality may be found in the auxiliary instrumentation developed for the detection of particles and processes initiated by these high-energy machines; some of these techniques are ahead of our own. Our visiting physicists in the Soviet Union were universally impressed by the excellence of the quality of engineering and instrumentation evident around these installations. A staggering number of different experiments set up at each machine exploit to the fullest the capabilities of each instrument. Few, if any, of these experiments, however, have been startling by their novelty. It is clear that no expense is spared in these enterprises. This is in stark contrast with the quality of the buildings, lighting fixtures, and other structures not immediately and directly connected with the scientific effort.

Even in the more speculative domains of still-higher-energy machines for the future, the Russian scientists are exploring unusual and highly imaginative schemes, some of which are evidently inspired by their efforts in the field of controlled thermonuclear reactions.

In my own field of low-energy nuclear physics or nuclear spectroscopy, however, I discern a considerable lag on the part of the Russian experimenters. Until recently, no results at all were forthcoming from any Van de Graaff electrostatic generators or cyclotrons in the several-million-electron-volt range of energies. In one special domain of nuclear physics close to my heart, for example, it was a Russian theorist who published the detailed theoretical basis of a certain excitation process of the nucleus. It remained the exclusive privilege of Western laboratories to confirm this theory experimentally, over the past three years, without *any* competition from Russian workers until less than a year ago. Such instances force me to the inevitable conclusion that special emphasis has been placed, in the Soviet Union, on the high-energy field, perhaps because of its greater glamor and international prestige. In the study of the decay of radioactive substances, on the other hand, a field requiring relatively few expensive instruments, the Russian physicists have been holding their own with respect to their Western counterparts since before the last war. In fact, they have even been using a number of detection schemes hardly used in other parts of the world.

Since my information comes mainly from the published literature, a word might be in order concerning this most essential adjunct of basic research. A great number of Russian journals report the Soviet findings. In addition, at least two West-European journals receive regular contributions in English directly from Russian laboratories. References are usually given freely and honestly to previous results by Western workers. The tremendous rate of Russian progress is easily gauged by the fact that, some years ago, perhaps 90 percent of all references given were to previous Western results, whereas today as few as 10 percent are in this category. In addition to the periodicals, great numbers of very useful monographs, compendia, lecture notes, and review articles are published, mainly at government expense, to judge from their low sales prices. Western scientific publications and books are speedily translated into Russian, and all visitors to Russia have been struck by the high degree of familiarity with Western results on the part of Russian physicists; the inverse situation certainly leaves much to be desired.

To sum up, then, I find that Soviet achievements in nuclear physics greatly overshadow those of other nations, with the exception of the United States, Canada, and Great Britain and of a few isolated laboratories on the European continent. In contrast to the situation in most other European countries, there seems to be a great pool of talented and trained manpower from which research physicists are effectively recruited.

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Agricultural Research and Production

Basic sciences. Soviet Agricultural research involving the basic sciences of chemistry, physics, soil morphology, general botany, and crop ecology formerly was adequate and of good quality, but most phases of biology have been distorted to conform with antiquated theories. Research in plant pathology in Russia has been very limited, but present interest in this subject is evidenced by the

reprinting of the U.S. Department of Agriculture 1953 *Yearbook*, which dealt with plant diseases.

Some excellent research in plant physiology has been conducted in Russia, but since about 1930 much of it, particularly that dealing with vernalization, has been so distorted by the theories of Lysenko as to be worthless.

Excellent genetic and cytologic research was in progress up to the time that Vavilov was banished and Lysenko assumed control of plant research in Russia (about 1945). Since that time, discoveries that have been reported often have been obviously completely false or of extremely doubtful validity; this has created doubts regarding the authenticity of much of the remainder. Reports that wheat plants produced rye or barley grains and that corn plants produced sorghum heads are beyond the realm of belief.

Practical accomplishments. Crop breeding has been only moderately successful in Russia, a serious handicap having been imposed by unsound genetic theories. The application of the science of genetics speeds up progress and reduces the work involved in crop breeding, although it is not essential to ultimate success. Experiments in crop culture and in evaluating crop varieties have been extensive and formerly were conducted satisfactorily, but are very crude according to modern standards.

Crop production. A year or two ago, Khrushchev announced that the Soviet agricultural program had been a failure. Russian agriculture is badly handicapped by the absence of profit incentives to the producers, the result of collectivization. The Yugoslav and Polish governments learned that fact in one year, and undertook no further collectivization. Government controls, also, are too inflexible to meet changes necessitated by seasonal fluctuations in weather and by fluctuations in the availability of facilities and equipment. Russia was a leading grain exporting country before World War I but since 1918 has had surpluses of grain only in a few very favorable years. Russian agriculture cannot be expected to reach modern standards until the profit incentive is restored, and that would sound the death knell of Marxist doctrines.

JOHN H. MARTIN
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Crystallography

Russian scientists are very active in many branches of crystallography. At the first postwar reunion of crystallographers in London, in 1946, the question of naming the proposed international journal of crystallography came up for discussion. The British delegation had suggested "Journal of Structural Crystallography," and the name had been agreed on by all but the Russian delegation. The latter remarked that, in their country, many phases of crystallography besides crystal-structure determination were actively studied—for instance, crystal geometry, crystal morphology, crystal growth, and crystal physics. Following this suggestion, the journal was called *Acta Crystallographica*. To our disappointment, no Russian paper has as yet been submitted to the journal.

With extremely few exceptions, all crystallographic papers by Russians appear in Russian journals. The most recent such journal in the field is *Kristallografija*, which the Academy of Sciences of the U.S.S.R. began publishing in 1956; its first six issues total close to 800 pages. Before this date, starting in 1945, works of the Institute of Crystallography were published in an annual volume, and, in addition, Russian crystallographic papers appear in a variety of journals dealing with physics, chemistry, geology, and metallurgy and also in the *Transactions of the Academy of Sciences*.

The Russians did not formally adhere to the International Union of Crystallography until 1954, the time of the Third International Congress, which was held in Paris. There the Russian delegation distributed hundreds of free copies of a special "Congress Issue" of the annual volume of the Institute of Crystallography. All the papers in this issue appeared in both Russian and French. At the Fourth Congress, held last summer in Montreal, the Russian delegation presented us with complimentary copies of a special issue of *Kristallografija*. Each paper appeared in Russian with a short English abstract. Another gift was a bound volume entitled *Crystal Growth* [375 pages; cost, 21 rubles, 40 kopecks (about $5.50)] containing 43 papers in Russian only. There seems to be no doubt in the minds of Russian crystallographers that we ought to learn their language!

In view of the quality as well as the quantity of Russian contributions, a reading knowledge of Russian has indeed become desirable. The thorough French abstracting service, during the first nine months of 1957, reviewed over 350 Russian crystallographic papers, which represent over 13 percent of the papers considered. The remaining ones came not only from the United States but from all other countries as well. It is interesting to note to what extent the Russians participate in the various fields of crystallography. As far as we know, they have not entered the field of neutron diffraction at all, whereas in crystal-structure determination by means of electron diffractions, they are leading by a wide margin. They are strong in the fields of morphological crystallography, crystallogenesis, and crystal chemistry (24 percent of 342 papers). Their interests appear to lie more in the theory than in its applications. They have neglected organic and biologic structure determinations almost entirely, although they have done a lot of work on minerals, metals, and alloys. Belov's laboratory is becoming a center for the structure determinations of silicates. The quality of this work has improved greatly in recent years, perhaps because of better instruments and computing facilities.

Only in the last few years have references to non-Russian publications appeared in Russian books and journals, but it has been evident all along that the Russians are up to date on what is being done elsewhere and, more important, that they make full use of all the findings of others. The same cannot be said for Western crystallographers. *Acta Crystallographica* contains almost no references to Russian works, and this is not the result of any editorial policy.

The U.S.S.R. is far ahead of the rest of the world in the teaching of crystallography. At least three universities (Moscow, Leningrad, and Gorky) have special chairs of crystallography. Specialists are trained in crystallography, crystal chemistry, and x-ray diffraction. They receive up to 680 hours, the equivalent of ten three-hour-a-week lecture courses and five three-hour-a-week laboratory courses in crystallographic fields. It is doubtful that this number of distinct courses in crystallography alone can be found in all the universities of this country combined. At the Madrid meeting of the Teaching Commission of the International Union of Crystallography in 1956, the Russians showed a movie of one of their courses dealing with crystal growth. Non-Russian members of the commission were speechless. No such course is taught in the United States. Where are these thoroughly-trained crystallographers employed? The best ones, is appears, work at the Institute of Crystallography, in Moscow. A Russian colleague who was asked how many full-fledged crystallographers were employed there answered, "about 200." There is no Institute of Crystallography in this country; indeed there is not even a department of crystallography in existence at any American university. Unless remedial action is taken now, American students of the coming generation who will ask their honest adviser, "Where can I get the best crystallographic training?" may have to be told the sad truth, "In the U.S.S.R."

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Earth Science

Russians have been active for many years in various branches of earth science, particularly in geochemistry. Recently, their effort has increased dramatically in many areas, particularly those related to the International Geophysical Year. The satellites are only one facet of their program. A notable example is oceanography, where numerous ships are being employed. There are, for instance, the *Ob* (a vessel of approximately 10,000 tons), a second ship of similar size, numerous big ice-breakers, and a nonmagnetic ship, all under the jurisdiction of scientists. In contrast, our studies in oceanography are conducted principally in two converted yachts of modest tonnage.

Russian activity in the field of seismology is intense and successful. The crucial problem in detecting a distant disturbance is to achieve a high ratio of effect to local background. The lower the background the more effective the observing equipment. The Russians have made a great effort to discover quiet sites and have studied the effects of time of day, weather, and season on these. Thus, they have been able to achieve a vastly superior sensitivity of detection for distant disturbances, amounting to as much as 50 decibels more than our best performances. Part of the answer is that our research people generally work from 9 A.M. to 5 P.M. The good Russian records were made at 3 A.M. Furthermore, the effort in seismology is well coordinated with an extensive network of some 60 stations. Our work is conducted by individuals, with a minimum of cooperation.

Another area where a contrast exists is in geophysical studies in the Arctic. We have two stations, both new—one on an ice floe, another on an ice island. The Russians have at least 12 such stations and have had several for years.

Two problems of immense importance to survival in the Arctic are those of water and permafrost. We have two geologists working on a study of these topics. The Russians have scores.

Studies of paleomagnetism—the magnetism of ancient rocks—were initiated in this country about ten years ago. Today only a few men here are engaged in research in that area, while in Russia more than a hundred scientists are active.

Another measure of the over-all Russian effort is the size of the Institute of Physics of the Earth, in Moscow. It is one of many such institutes devoted to geophysics. Stationed at the Moscow institute alone are 300 scientists, a number which considerably exceeds the total in the whole United States of those doing fundamental research in geophysics.

In almost every area of geochemistry the volume of effort at least matches our own. In my particular specialty, geochemistry of organic materials, the effort of the Russians is perhaps four times as intense as that we are making. Schairer of our laboratory has recently reviewed papers in heterogeneous equilibria. He reports much activity in that area, particularly in studies related to earth science.

The activities of Russian geologists have been well supported. One of the problems of a field geologist is that he can often examine readily only the surface rocks. In areas where economic considerations lead private enterprise to drill, the U.S. Geological Survey may examine cores to obtain detailed information about subsurface conditions. Unfortunately, drilling for economic reasons is limited to a fraction of the area of the country. For years our Geological Survey has wanted to drill some holes where the purpose was simply to learn about the geology of a given region. Funds have not been available for this purpose. The Russians have a program for drilling 200 holes each year, at places selected by research geologists. These holes are located without reference to economic considerations and go through the surface layers down into the basement complex 1000 feet, with total depths often in excess of 10,000 feet.

In summary, it appears that, quantitatively, the Russian effort in fundamental aspects of earth science far exceeds our own. From the viewpoint of quality, the story is somewhat different. Part of the Russian work is excellent. Some is poor. For the most part, its quality is comparable to our own. A Russian geophysicist who recently visited many of our establishments in this country was amazed at the quality and quantity of our research output in earth science in view of the small numbers of active workers and of the evident lack of organization and support.

An important problem we face is that of communication of scientific ideas. Our scientific periodicals are speedily translated, and the Russian scientist often receives copies of our journals in his own language about as fast as we do. Thus, Russia has the benefit of our scientific progress plus its own. Although our translation program is improving, costs are still too high for the achievement of adequate widespread distribution among the working scientists of this country.

Another handicap is the relative cost and availability of technical books. Recently, a 15,000-copy edition of an 800-page book of geotectonics was published in Russia, selling for the American equivalent of about $3. The edition sold out within a month of publication. Were a similar book published in this country, it would sell at about $20. During the first year, 1000 copies might be sold, with perhaps 300 purchased during the first month, mainly by libraries.

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Soviet Psychology Since 1950

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To judge by Russian bibliographical sources—*Novyye Knigi, Letopis' Zhurnal'nykh Statey, Referativny Zhurnal, Sovetskoye Meditsinskoye Referativnoye Obozreniye,* and the annual bibliographies in *Voprosy Psikhologii*—57 books and 513 articles of a technical psychological nature have been published in the Soviet Union in the present decade. About 91 percent of the publications appeared in Russian and the remainder in Ukrainian, Georgian, and Belorussian. In the latter three languages only a small sample of original material was available for examination, and the coverage of Georgian was rendered further inadequate by the fact that, not reading the language, I had to content myself with appended Russian abstracts. However, the psychological publications in Russian itself were both absolutely and relatively plentiful: 45 books and 414 articles, 89 percent of what is known to have been published. Since the Geneva Conference and the Eisenhower-Nixon declaration on cultural exchange, the proportion of current Soviet scientific literature in our libraries—notably the Library of Congress, the National Library of Medicine, the Library of the Department of Agriculture, and the larger university libraries—has signally increased, even as private procurement of such literature has become much easier, with little interference from our customs officials and readier response from Soviet publishing houses. (The paucity of Soviet scientific literature in languages other than Russian is no doubt merely a matter of temporary delay in supply and demand that will presumably, other things being equal, become normalized in the near future.)

Pre-1950 Background

Appraisal of Russian psychology of any period must first clear up some formal problems of scope. Traditionally, as a separate and specifically delimited discipline, psychology never has been—and is not—independently significant in Russia's matrix of learning. Yet, as a general area of research and thought, it has always been—and is—in the forefront of Russian intellection. Psychologizing and psychophysiologizing—that is, attempting to explain psychological phenomena in physiological terms—have indeed long been favorite preoccupations of a number of influential Russian intellectuals, quite irrespective of formal affiliation with psychology or even with science in general. Consider the extrapolative "psychological" views of the anatomist Lesgaft, the bacteriologist Mechnikov, the botanist Timiryazev, the geochemist Vernadsky; the pungent psychophysiologizing in the sociological, political, and literary essays of Radishchev, Hertzen (Gertzen), Dobrolyuobov, Belinsky, Chernishevsky, and Pisarev; and the extrapsychologizing of so many prominent Russian novelists. The first Russian laboratory in experimental psychology was set up by Korsakov, the well-known psychiatrist; the first serious utilization of mental testing was made by Rossolimo, another psychiatrist (Rossolimo's "psychological profile"); and the chief Russian delegate and honorary member of the presidium of the First International Congress of Psychology, held in Paris in 1889, was the physiologist Sechenov, about whom more is said in the next paragraph. Incidentally, there were at this first congress 19 Russian delegates—and three Americans: William James, Joseph Jastrow, and someone by the name of Riley. For the life of me I do not know who Riley was!

Now, if you look into almost any English medical dictionary under *Sechenov* (or *Sechenov's inhibition, Sechenov's inhibitory center*) you will find: "Ivan Mikhailovich, 1829–1905, father of Russian physiology and neurology"—which is correct. But what is even more correct is that Sechenov is the father of Russian psychology. Note the titles of his larger publications: *Elements of Thought, Impressions and Reality, How and By Whom Should Psychology Be Studied, Object-Thought from a Physiological Standpoint, The Doctrine of the "Un-Freedom" of the Will,* and the classic monograph *Reflexes of the Brain.* Published in 1863, the monograph—127 pages—contains almost all the basic ingredients of future reflexology and behaviorism, including a very modern, almost Skinnerian, conception of the reflex and including a view that perceptions and thoughts are centrally inhibited reflexes developed by the individual in the course of early postnatal ontogenesis which has—or could have—Freudian implications (*1*). There is no doubt that Sechenov is Russia's all-time most psychologically involved physiologist and the true founder of its special school of psychology. Pavlov and Bekhterev, particularly the former, later provided, to be sure, the experimental foundation of the school. But Sechenov continues to be its chief theoretician. Unlike Pavlov, who presumably felt that he could in the main ignore the analysis of basic psychological categories (or felt that he was insufficiently familiar with them), and unlike Bekhterev, whose analysis of these categories is rampantly naive and speculative, Sechenov tackled directly and fully almost all of psychology's perennial problems and tackled them with a sophistication—psychological and philosophical—that is, to the present day, striking and provoking—and highly systematic.

But here a qualifying statement is called for. What has been said so far is by no means intended to imply that all Russian psychological research and thought have come from its psychologizing physiologists and psychiatrists. Russia had had a very respectable group of pure psychologists, who compare favorably with those of other countries: Troitsky, Lopatin, Chizh, Lange, Nechayev, Lossky, Lazursky—and I could name another dozen, I suppose. It had psychointrospectionists, psychorationalists, psychointuitionists, and psychologies of *becoming,* of *self-actualization,* and of *anticipation.* But, unfortunately, these psychologists and psychologies never "became," are no longer "actual," and "anticipated" poorly. Their line of development has been forcibly cut off during the Soviet era, so that a discussion of them here could be of only very limited and extraneous relevance. Moreover, it is also true that, good as those Russian psychologists were, they never attained the eminence of the psychologizing physiologists. No Russian psychologist proper has, for instance, ever been a member of the Russian Academy of Sciences, either before or since the Revolution, whereas the psychologizing physiologists and psychiatrists have almost always dominated the biological and the medical sections of the Academy: Sechenov and Korsakov earlier, then Pavlov and Bekhterev, and now Bykov and Ivanov-Smolensky.

Continuing our discussion of back-

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ground, we obviously must take some note of what happened to psychology in the Soviet Union before 1950—a complex and tortuous zigzagging that space forces me to compress into five short paragraphs.

1) At the beginning of the Soviet era, Pavlov's physiology, Bekhterev's reflexology, and imported Watsonian behaviorism quickly became the dominant school or schools of psychology (the three are quite similar but not identical), routing in their wake all other trends as subjectivistic and idealistic. The schools' theoretical resultants at times led, under Marxian ministry, to such absurd formulations as "consciousness is a product of the inhibitions of the capitalistic system and will wither away under socialism." Yet those schools surely managed to stimulate a large amount of significant research and thought in the early '20's, and psychology at that time bade fair to become a basic science in the Soviet State.

2) By the middle '20's, however, the entire Pavlov-Bekhterev-Watson combination, as a system of psychology, fell under the sharp attack of top Soviet theorists. Nicolai Bukharin, at that time at the height of his power and the acknowledged theoretician of dialectical materialism, began the attack in 1924 (*2*), and others followed him (*3*) in plainly declaring that Pavlov and Bekhterev study correctly only the physiological basis of the mind, but not the mind itself, that conditioned or associated reflexes are physiology and not psychology, and that Soviet psychologists must develop a *sui generis* dialectical Marxian and materialistic psychology and not just coalesce with physiology, reflex study, and muscle action. The attacks produced their effects. By 1930 we find, in the periodical *Psikhologiya,* an article entitled *The Beginning of the End of Reflexology* and, by 1932, another article declaring that reducing psychology to physiology, reflexology, or behaviorism is a Menshevistic and Trotskyistic deviation.

3) For a while, in the late '20's and early '30's, there was a mild flirtation between Soviet psychology and Gestalt psychology and also, to some extent, psychoanalysis. But this was very short-lived, and by the middle '30's and the period of the purges, it became quite evident that the key official desideratum for a Marxist-Leninist-Stalinist psychology was that it should be as different as possible from any existent "bourgeois" psychology, and Soviet psychologists began vying with each other to see how different they could get.

4) The attempt to create a Soviet psychology without Pavlov and without "bourgeois" psychology proved a total fiasco. The mountains labored and produced puppies. And as a separate discipline, Soviet psychology was in continuous decline after 1936; it had no periodical of its own and produced little new research and thought and only one significant book—that of Rubinstein (*4*)—which, too, was condemned only a few years after its publication.

5) While psychology was languishing, research in conditioning and related phenomena continued unabated. Pavlov's death in 1936 halted largely, it is true, new theoretical excursions, but, if anything, it increased the pace of actual experimentation. In 1949, the country celebrated with considerable pomp the 100th anniversary of Pavlov's birth; and, as is well known, in the late '40's, Stalin decreed a sickly xenophobia and a Russification and "partyization" of science and culture. Hence the events of 1950, which begin the main theme of the present article.

1950 and Pavlovianization

What were the 1950 events? For 7 days, 28 June through 4 July, the Soviet Academy of Sciences met jointly with the Soviet Academy of Medical Sciences to "discuss the problem of the physiological teachings of the all-time 'coryphaeus' of all the natural sciences, Academician Ivan Petrovich Pavlov"; specifically, to "reconstruct on the basis of Pavlov's teachings: physiology, psychology, pathology, psychiatry, clinical medicine, pharmacology, immunology, animal ecology, animal husbandry, pedagogy, physical education and the science of health resorts." (The last is named *Kurortologiya,* the Russians using the word *Kurort,* of German origin, for health resort; it is hard to see why the subject of health resorts suddenly acquired the status of a full-fledged basic science except for the fact that Stalin was at that time much interested in it—and much in need of it!) One hundred and forty-two papers were read at the meetings by the Soviets' top scientists, almost all in the same vein: criticisms and self-criticisms for not having seen the light of Pavlov's teachings and for not having used it in one's scientific and professional endeavors, avowals to mend one's ways and adhere to the light thereafter and therefrom, and resolutions to set up Pavlovian Committees to guard the light and see that others see it (*5*). Twelve meetings of the Academies' Pavlovian Committee have been held since, besides a number of special Pavlovian Conferences convoked by individual scientific societies.

Let us see, then, what these meetings, committees, and resolutions did to Soviet psychology—and we might begin with an examination of general textbooks, which, as one would suspect, are, in the Soviet Union, more than any other publications, matters of particular circumspection and of official sanction and approval. I thus have by me eight textbooks of general psychology, six published after 1950, one in 1948, and one in 1940 (*4, 6*). Multiple textbooks on the same subject are uncommon, and there is a special reason for the six current ones. Each of the six is designed for a somewhat different audience: one for military schools, another for students of physical education, a third for pedagogical institutes, a fourth for kindergarten teachers, a fifth for secondary schools, and the sixth unspecified. Incidentally, the textbook designed for secondary schools, *Psikhologiya* by B. M. Teplov, was printed in 1,100,000 copies, and this printing was only of the eighth (1954) edition. (Previous editions were issued yearly, beginning with 1947, and the circulation of some of these was not, as far as I could ascertain, much smaller.)

Now, even a mere cursory comparison of the textbooks published before and since 1950 reveals that the latter are thoroughly steeped in Pavlovian doctrines while the former are hardly touched by them at all. More specifically, a detailed analysis of direct quotations in six of the eight books (the more elementary secondary-school- and kindergarten-teachers' texts had too few quotations to be included) shows that direct quotations from Pavlov range from 22.9 to 41.7 percent of the total (absolute numbers, 19 to 30) in the since-1950 books but that they are wholly negligible in the pre-1950 ones: 1 to 4, 0.7 to 3.9 percent. Again, while the 1940 textbook contains 41 quotations (28.9 percent) from non-Russian scientists (almost all psychologists) and 36 quotations (25.4 percent) from Russian scientists (almost all physiologists, psychologists, and psychiatrists) other than Pavlov, there are in the books published since 1950 absolutely no quotations from any non-Russian (except Marx and Engels) and there is a mean of only 7.5 percent of quotations from Russian scientists other than Pavlov.

Clearly, Soviet textbooks of general psychology have become radically and drastically Pavlovianized since 1950. Yet it would be misleading to say that Pavlov's teachings are their sole pabulum. There are also, of course, Marx, Engels, Lenin, and Stalin, whose relevance to, and omniscience about, the science of psychology no writer of a Soviet textbook could, presumably, afford to doubt and ignore, as indeed none did. The mean percentage of direct quotations from these authors in the four current textbooks is 45.9(!), surely justifying the recapitulation that, at least as far as source material and documentation are concerned, these current texts are based

on Pavlov, on the one hand, on so-called Communist classics, on the other, and on practically nothing else.

Drastic Pavlovianization since 1950 is also unmistakably evident in areas of more basic research and thought, as represented in scientific and professional periodical articles and theses for the doctorate. My list of titles and abstracts of Soviet doctoral theses in psychology is, unfortunately, fragmentary: 15 between 1928 and 1950 and 22 after that date. Yet the differences between the two periods are so wide that a null hypothesis would not, by any sign test, be at all tenable. Before 1950, only one of the 15 theses was related to Pavlov's teachings, and it, too, was admittedly critical of them, at that, whereas since 1950 no less than 17 of the 22 have been seemingly wholly based upon these teachings—have had the name of Pavlov or of his doctrines in the titles—with obviously no recognizable modicum of animadversion. My source material for comparing articles in psychological periodicals is, on the other hand, almost complete, even though, because of the nonexistence of such periodicals in the Soviet Union between 1936 and 1955, the comparison must of necessity be confined to articles published in the late '20's and early '30's (in the periodicals *Psikhologiya, Sovetskaya Psikhotechnika,* and *Sovetskaya Psikhonevrologiya*) versus those that appeared in 1955 and 1956 (in the periodical *Voprosy Psikhologii*). Result: I found that less than 6 percent of the articles involved Pavlovian research and views in the earlier period but that less than 4 percent of the articles did *not* involve such research or views, or both, in the latter, present period.

A word might also be said about the exclusion, often more correctly nonidentification, of non-Russian contributors and contributions. This Russification, begun some time in the middle '40's and revealed, in the case of periodicals, not only by the absence of non-Russian references in bibliographies of articles but also by the omission of customary non-Russian (usually English) summaries of the articles, has, it should be noted, been tempered considerably in the last year or so. Both non-Russian summaries and non-Russian references are beginning to reappear in a good number of Soviet scientific periodicals, and recently *Voprosy Psikhologii* has even instituted in each of its issues (bimonthly) a special section on "Psychology Abroad."

The fact that Soviet textbooks of general psychology contain now no more than a very few quotations from Soviet physiologists and psychologists other than Pavlov is of special significance and calls for further discussion. It signifies a very particular aspect of Pavlovianization—namely, its extreme orthodoxy. Indeed, castigating and bringing into the fold of Pavlovian research and thinking Soviet scientists who, prior to 1950, had been outside the fold was only one of the two tasks of the afore-mentioned meetings of the Soviet Academies and their Pavlovian Committees. The other objective was to see that Pavlov's research and thinking are correctly interpreted and to castigate Soviet scientists who, within the fold of Pavlovian experimentation, had propounded interpretations and theories that, according to the leaders of the meetings of the Academies and Committees, are incorrect and deviant. Or, to put it somewhat more strongly, current Soviet thinking is Pavlovian, not in the sense of being based upon what Pavlov did and stimulated others to do—that is, upon a free and broad critical analysis of experimental evidence of classical conditioning and related phenomena—but in the sense of conforming with what Pavlov said and what present "official" exegetes of his system say he said. I should delete the word *say*—Pavlov was not rewritten, he might have been, but he was not—and substitute the word *interpret*: ". . . what present 'official' exegetes of his system interpret him [Pavlov] to have said."

Deviant Interpretations

More specifically, the Academies' and Committees' castigations were directed, in the main, against four outstanding Soviet psychophysiologists and lifetime students of Pavlovianism: I. S. Beritov (Beritashvili), by all tokens Russia's most important neurophysiologist, next to Pavlov; L. A. Orbeli, Pavlov's chief pupil and, until 1950, his acknowledged successor; P. K. Anokhin, a favorite pupil of both Pavlov and Beritov; and P. S. Kupalov, with whom the American physiologists W. H. Gantt and H. S. Liddell had worked.

Beritov was an early heretic. While accepting and largely verifying Pavlov's main behavioral findings, he early set himself against the assumption of internal inhibition (rechristened in this country, by Hull and others, "reactive inhibition") and Pavlov's special views of cortical irradiation and induction. And later on Beritov went much further. Discarding the Pavlovian harness and experimenting instead with free-moving animals in a Graham-Gagné runway [the Russians would resent my describing the set-up as a Graham-Gagné one, claiming that they used something like it before (as indeed they did, in 1913) (*7*)], he arrived at what might be regarded as a two-factor view of learning: Pavlov's laws of conditioning hold when animals are in harness and the *environment does something to them,* but new laws are needed for free-moving animals who *do something to the environment.* It is a view—a distinction between reflexes and what Beritov calls psychoneural behavior—that is not unrelated to the views of the American psychologists Woodworth, Tolman, and Skinner, and certainly it would be reputable here. But it got Beritov into all sorts of troubles; whole sessions were devoted to taking him apart, he was summoned to attend the sessions, committees were sent to his laboratory, his own students denounced him—until after a few years he succumbed and recanted: "I was wrong."

Orbeli's sins were, in a large way, less in the realm of basic theory. He was primarily accused of not advancing sufficiently the study of conditioning, of overemphasizing the unconditioned as against the conditioned action of the sympathetic nervous system (which is his specialty), and of kow-towing to foreign scientists. In one of his lectures, Orbeli said that, "while watching a house at a distance and observing those who enter and leave it offers a good picture of what goes on in the house, one should really also live in the house to complete the picture"; this was denounced as a subjectivistic simile and haunted him for some time. And, again, he was severely criticized for attempts to replace Pavlov's concepts of conditioning as a signal system by a deviant and separatist concept of his own. Like Beritov, Orbeli refused at first to recant but finally did.

Anokhin's deviation was the most interesting one. What he did was to modify the Pavlovian harness so as to feed his dogs not from one side but sometimes from the left and sometimes from the right (*8*)—and that was bad: differences between left and right in a classless society! Seriously, it was of course the fact that Anokhin concluded that Pavlov's system could not account fully for the behavior of his dogs in what he called the "active choice" situation and suggested some supplementary concepts. Anokhin recanted quickly and published a *mea culpa* (*9*), as did also Kupalov, who for a while advanced a theory of truncated conditioning—that is, conditioning that does not result in effector changes (something like sensory conditioning or like Lashley's view of thinking as contrasted with Watson's) and conditioning that is centrally initiated in a manner not unrelated to the mechanism of ongoing neural patterns, as expounded by Hebb (*10*) and others. Kupalov is now presumably fully rehabilitated: since 1954 he has been chief editor of the main psychophysiological periodical on conditioning, *The Journal of Higher Nervous Activity.*

A Fixed System

Pavlovian psychophysiology has thus acquired in the Soviet Union, it may well be said, a status that, at least with respect to basic theory and principles, is more like that of an established and not-to-be-challenged ideology or philosophy than that of a system of an experimental natural science. Not only are methodologies and interpretations that deviate in any way from this psychophysiology discarded or rather suppressed but the system itself lost much of the plasticity and organic growth that it had when Pavlov was alive. Dedicated and biased toward his own ideas as Pavlov was—and he was really not more so than our own system-makers—he was nonetheless, in his own words, at all times an experimentalist from head to foot, basing each of his generalizations upon specific and well-controlled experiments of his numerous students and unhesitatingly introducing, whenever warranted, new generalizations to modify and even supplant old ones. Moreover, there is good evidence to believe that a true democratic give-and-take prevailed in Pavlov's laboratory and that at least some of the pillars of his system had their origin in his students' minds, so that, while in its entirety the system is surely the edifice of Pavlov's own genius, it is, nonetheless, also a cumulative collective enterprise.

Compare all this with the present—in which modification and revision are heresies to be shunned; when not a single new principle of basic theory has been added in more than 20 years, despite a doubling in volume of experimentation and the emergence of whole areas of novel empirical findings; and when conformity with some quotation from Pavlov is, as a rule, more of a recommendation of the validity of an experiment than is its own intrinsic worth—and you can hardly fail to note the encompassing aura of the cult of Pavlov's personality and the consequent conversion of a set of open and free hypotheses into a code of closed and forced dogmas. Like Marx, Engels, Lenin, and (until recently) Stalin, Pavlov is now a Soviet classic, endowed with almost theologistic prescience of certainties and surely above criticism and impugnations—a doctrine which Pavlov himself, the thoroughgoing empiricist and antidogmatist, fought all his life and, indeed, considered ridiculous.

Content Analyses and Problems of Synthesis

Having now delineated the formal properties of current Soviet psychology, I feel that an analysis of its contents is in order. And here, again, the textbooks of general psychology might serve as a start, particularly when a mere glance at their contents discloses a peculiarity that poses questions of basic and comprehensive significance. Despite the drastic Pavlovianization, the psychological categories which the current textbooks treat, roughly indexed by titles of chapters, hardly differ from those in B.C. ("before conversion") texts—and indeed have remained almost unchanged since the decline of pure reflexology in the mid-'20's. Moreover, these textbook categories or chapters are much more like what one finds in Woodworth of 1921, Pillsbury of 1918, and even Angell of 1906 and Höffding (Danish psychologist) of 1891 than in any contemporary American equivalents. The traditional categories of sensation, perception, attention, imagination, memory, feelings (occasionally emotions), will, language, thought, habit, and action are accorded separate and dominant chapters, in post- as well as in pre-1950 texts, whereas the more recent and not-so-recent categories of motivation, adjustment, and intelligence—our own *pièces de résistance*—are seldom mentioned even as mere concepts and problems. (Other chapters in Soviet textbooks are on physiological foundations, development (usually entitled "Development of Psyche"), and personality—and, since 1950, small sections on physiological foundations, that is, Pavlovian physiology, have been interspersed in almost all chapters.)

Surely all this must be very surprising: (i) Why retain in full bloom the traditional categories in a Pavlovian psychology when Pavlov himself was so manifestly concerned with scrapping not only the categories but the whole of psychology as well? (ii) Just how do Pavlovian principles synthesize with traditional categories, and how have the Russians done it? And to complete the roster: (iii) Where does the other half of Soviet psychology, Marxism-Leninism, enter into the synthesis? And, finally, (iv) what exactly is the nature of the actual and specific work of Soviet psychologists and psychophysiologists and how is this work specifically affected by Pavlov, traditional psychology, and Marxism-Leninism? I shall attempt to answer the questions in sequence.

The answer to the first question is simple to state, though no doubt difficult for a non-Russian-, or rather non-Marxian-, reader to assimilate. It is the fact that the *objective of Soviet theorists and the Soviet State was to add Pavlov to Marx, Engels, and Lenin but not to supplant them.* And Marx, Engels, and particularly Lenin, explicitly emphasized the active role of consciousness in transforming nature, society, and man and in changing, in Lenin's words, the "thing-in-itself" into the "thing-for-us"—whatever the phrase means. In more formal terms, the credo usually reads (to epitomize phrasal variations of a recurrent theme): "While the psyche is but an attribute of highly organized matter—the brain—and is secondary and derived in a cosmologic and genetic sense, it is, nonetheless, a true ontologic reality the 'within' categories of which must not be liquidated [that is the term] by 'without' categories of brain action." More simply, I would say that 19th century and early 20th-century concepts of purpose, will, and imagination and of consciousness itself are too inveterate in Communist philosophy, ideology, and propaganda for any mere psychologist to dare derange them. The Politbureau, the Central Committee of the Communist Party, or some future Communist Congress might conceivably some day initiate such an act, but not just psychologists. And another factor: Soviet psychologists, unlike Americans, may not indulge in any kind of operational definition or conversion of consciousness and its categories. For operationalism, they say, is positivism, and positivism is Machism, and Lenin condemned Mach as an idealist long ago (in *Materialism and Empiriocriticism,* 1909). So, Soviet psychologists are really legislated into consciousness, stuck with it "as is" so far.

But, then, what about Pavlov? Was he also a dialectical materialist and an adherent of the view of specific efficacy of consciousness and psyche as such? The answer is, of course, "No." Pavlov repeatedly contended that his study of higher nervous activity, which he equated with the study of behavior—the first of his three books on conditioning is entitled *Twenty Years of Objective Study of the Higher Nervous Activity (Behavior)*—was intended (really destined) to wholly supersede older and subjective methods, and when he used the term *psychic* he typically designated it as *so-called psychic*; the title of his famous 1906 Thomas Huxley lecture is, for instance, "A Scientific Study of the So-called Psychic Processes." And in one of his last articles (1935) he explicitly stated: "The adjective *higher nervous activity* corresponds to the adjective *psychic,*" and, "What basis is there then to separate one from the other, to draw distinctions between what the physiologist calls a temporary connection and the psychologist—an association? There is here a total blending, a total engulfing of one by the other, an identity" (*11*). (obviously an engulfing of psychology by physiology). Then, in discussing Kohler's experiments and views, Pavlov further said: "You see, it seems as if he [Kohler] is now coming around to our point of

view. Consciousness [he says] 'enthalt in sich keine besondere Kraft' [possesses no special force]" (*12*, p. 20).

There is no doubt that with respect to psychology Pavlov could be classed as nothing but a behaviorist—indeed, in a way, the true progenitor of behaviorism and, in a way, more so of the current-day than of the Watsonian variety—and with respect to philosophy as an epiphenomenalist, which is exactly how Soviet theorists considered him 30 years ago. But now they have decided to change and say that he is different, "the greatest and truest champion of dialectical materialism in the natural sciences." These theorists are experts in explicative reversals of their own former positions, under exigencies, and in "reconciliation of opposites" by .fiat, and their reasonings are really not matters of evidence and logic. George Orwell's *1984* might well be of more value here.

The answer to the first question, together with some of my earlier remarks, silhouettes also, it is thought, the answers to the second and third questions, which, it may be said, should be regarded as largely negative. The Russians have in no significant way succeeded in synthesizing Pavlov with the traditional psychology of conscious categories, for the mere reason that synthesis demands modification of what is synthesized and no such modification is possible under their present system. Pavlovian orthodoxy permits no change in Pavlovian principles—a change, let us say, that would recognize several levels of learning of which classical conditioning is one, or that would view conditioning as primarily a perceptual process, or that would admit a concept such as semantic conditioning. And dialectical materialism or Marxism-Leninism forbids abridgment of the active role of consciousness or reconceptualization of its categories, or both—forcing modern research and thought in the area into predesigned and for-decades-unrevised molds—and forbids, of course, modifications in its own socioeconomic premises. Or, in other words, what the Russians have produced is, on the one hand, (i) a mere unsynthesized juxtaposition of Pavlovian and traditional psychological principles and findings and, on the other hand, (ii) a lot of selected quotations purporting to prove that Pavlovianism and Marxism-Leninism imply each other; that is, that Pavlov meant what Marx, Engels, Lenin, and Stalin said and that Marx, Engels, Lenin, and Stalin meant what Pavlov said—a casuistic conflation of texts if there ever was one. Moreover, inasmuch as adherence to the Soviet psychological system is not a matter of free choice, specific interpretations of Soviet psychologists often manifest more lip service than true evidence of an organic integration with their general system. Indeed, not infrequently these interpretations, whether from ignorance or willfully and *sub rosa*, transgress the system altogether by incongruous or even contradictory theses and professions. Prohibition has been known to lead to bootlegging.

Theoretical Framework

Yet it would be a great mistake to underestimate the achievements of recent Soviet psychology, and it would be correct to say that, while languishing before 1950 and at first quite shaken up by the traumatic about-face conversion to Pavlovianism, it has, in the last 2 or 3 years, straightened out and is resurging rapidly in practice and even in theory (that is, Pavlovian theory proper, its synthesis with conscious categories and dialectical materialism being, as has just been stated, of little systematic significance and, in the main, merely proclamatory). Let me begin with theory, dividing the entire framework into eight brief parts or principles:

1) "Classical conditioning is the functionally exclusive principle of animal and human modifiability, modifying effectively and radically all reactions from the simplest viscerovisceral reflexes to the most complex human values and judgments which, too, must be conceived reflexively." This is of course the principle of the universality of associationism, differing in its very essence but little from the theoretical positions of a very respectable number of American psychologists.

2) "Classical conditioning is in itself a psychic act. Lenin said that the essence of the psyche is its ability to reflect reality, and conditioning enables a fuller reflection of it, and, moreover, at higher levels correlates with and gives rise to conscious perceptions and images." Lenin's reality-reflecting is, of course, here nonessential, and the statement could readily be changed to read, "Conditioning is a mental or minded act in producing better adjustment," with its familiar American ring, while there obviously could be no objection to endowing association (or conditioning) under certain conditions with some power of "creative synthesis" (Wundt).

3) "A large portion of conditioning, especially that of interoceptive conditioning, is, though psychic, unconscious in essence, and this unconscious conditioning is all there is to the bourgeois concept of unconscious motivation and psychosomatic effects and actions." Little need be added here except to say that "unconscious" is not Watson's "unverbalized" but our more recent "subceptive" (that is to say, consciousness is not wholly identified with verbalization in current Soviet psychology).

4) "Conditioning of the second signal system and its interaction with that of the first is the basis of human thinking and other higher mental processes." Here special explanation is needed. In his later years, Pavlov stated that language is a second signal system, adding that "words are by their very nature abstractions" and "signals of signals." Pavlov did not base this statement, as was usually his custom, upon any specific experiments (although experiments in the area were by that time available) but uttered it in a general extrapolative fashion. Still, the statement turned quickly into a fountainhead of psychological theory and has since given rise to scores of exegetic treatises. Language, the second signal system, is not, it is contended, just second-order conditioning or conditioning to another kind of stimulus but is a new, special, higher, and abstracting kind of conditioning. And, mind you, it is new and special and higher and abstracting not because of its semantic aspects (semantics is a taboo term, related to positivism and Mach) but is so in its entirety. Moreover, since the Russians profess also a traditional view of a lower and simpler consciousness of sensation and perception and a higher and more complex consciousness of thinking and imagination and, in addition, favor the socioecological distinction between adjusting to an environment and changing it, they thus unfolded another set of parallel formulations. Simple or first-signal system conditioning is the material basis of sensations and perceptions and is the animal means of adjusting to the environment, whereas second-signal system conditioning is the material basis of thinking and imagination and is the human tool for changing the environment. Material basis, it must further be remembered, does not mean equality or sameness. Soviet theorists exult in the view that the unity of the brain and the mind does not mean identity, and they would likewise insist on saying that the unity of language and thought does not mean identity.

5) "In the interaction between the first and second signal systems, the second system typically directs and dominates, but it is the first which is basic and real; the second system alone lacks reality and reference, even as the first alone is devoid of human content and direction." This is an important principle, with both theoretical-ontological and practical-psychiatric implications, but one that is still largely in an early programmatic stage.

6) "The manner of the interaction between the first and second signal systems and relative role of each, combined with the genetic strength, mobility, and

balanceability of an individual's cortical processes, determines both the general type and the unique characteristics of his personality." The specific corollary of this principle, the Pavlovian typology of personality, is, to my mind, the weakest link in Pavlovianism—and one with which, I discern between the lines, even some Soviet conditioners are discontented (it figured but little in the recent All-Union Soviet Conference on Personality)—so that I am a bit surprised that a number of British and American psychologists have recently embarked upon its validation.

7) "Except in lower animals, all conditioning involves cortical action and the operation of specific cortical mechanisms of excitation, inhibition, irradiation, and induction; but the absolute, and relative, roles of each specific mechanism vary considerably among various types of conditioning, various individuals of the same species, and various species." This is, of course, Pavlovian cortical neurology, to which the Russians accord empirical scientific reality but which might better be classed, so far, as largely C.N.S. (Conceptual Nervous System), to borrow a term from Hebb. Pavlovian inhibition and irradiation (generalization) have been subjected to a good deal of experimentation in this country, with—to be generous—varying results, but, interestingly, induction, despite its history in sensory psychology, has, for some reason, never been exploited here.

8) "Classical conditioning is not just a mechanical system of concatenated connections, their waxing and waning, but under certain circumstances, especially in higher animals and man, gives rise to a characteristic dynamism named the 'dynamic stereotype.'" This concept was developed by Pavlov in his later years on the basis of a large number of experiments in which groups of ordered stimuli came to affect, significantly and radically, the course of later conditioning. The concept is really quite similar to Harlow's *learning sets* and to my *attitudes* (in conditioning), though Pavlov himself seems to have preferred to think of it as a counterpart of *Gestalten*. In his own words, "'dynamic stereotypes' prove that associations generate and govern *Gestalten* and not *Gestalten* . . . association" (*12*, p. 46).

As may be seen, the framework of the eight principles is almost wholly a Pavlovian systematization with but a small sprinkling of nonspecific Marxist-Leninist expressions (nonspecific in the sense that they may be found also in the writings of non-Marxians). The occasional mention of consciousness and conscious categories is purely descriptive and in no way constitutive of any essence of the systematization itself. Indeed, there is nothing in the presented systematization to which an American behaviorist *qua* behaviorist could object, reaffirming fully, as it does, the earlier expressed view that what the Russians have is a Pavlovian psychology which in itself is complete and autarchic, and that whatever else they introduced into the system is insignificant and extraneous and stems, not from empirical and logical analyses, but from dicta of a handed-down and unwilling-to-be-modified philosophy which, by its very nature, contravenes scientific psychology—and Pavlov.

Soviet Psychologists' Actual Work

Conditioning being the very core of current Soviet psychological theory, one would expect that it would also account for the lion's share of the actual work in the field (our fourth and final question). It does. The scope of experimental work in this specific area is indeed singularly impressive. Last year I counted a total of 1507 separate reports of experiments in classical conditioning that have come out of Russian laboratories. My present count includes a few hundred more, and I should add that, by all tokens, the technical quality of the experiments is of very high caliber.

The Russians attached radio transmitters to their dogs to signalize electronically conditioned salivation as early as 1931 (*13*), and in recent years a good number of conditioning studies have been accompanied by electroencephalographic records. Pavlov, as is well known, was a very skillful surgeon, and surgical skill seems to have been fully kept up and cultivated by his pupils, almost as a sort of distinguishing—and distinguished—laboratory coat-of-arms. Just as Pavlov brought through the body's surface, or surgically exteriorized, the ducts of the salivary glands for the purpose of objective experimentation, so do his pupils exteriorize for the same purpose the ducts of other glands and the internal organs themselves; they have, indeed, succeeded in a two-way conditioning of almost all the viscera. You may condition a dog to contract his spleen when a bell rings just as you may condition him to lift his paw when the spleen contracts (the latter variety is the one called "interoceptive conditioning"). This is no mean achievement, and it would be folly to minimize its tremendous applied and academic significance for physiology, psychology, and medicine—in general, and in their development in this country in particular. The techniques should be learned, the information mastered, and the experiments repeated.

To be sure, all this is the work of the physiologists. The psychologists proper have not done so well. For one thing, as many readers may know, research and even free discussion of a significant section of modern psychology have been *verboten* for some time. Mental testing and indeed any interindividual psychometrics and evaluation went out of existence in 1936 (through the official Communist Party decree on "pedological inversions") and with them went, as one might expect, most phases of what we commonly class as social and industrial psychology. For another thing, prophylactic aspects of mental health, which beget and nourish so many American psychologists (clinical psychologists), and related interests in motivation are in general underdeveloped in the Soviet Union, partly because of its specific socioeconomic conditions and philosophy but partly also, no doubt, because of the long-standing ban (unofficial but real) on the teachings of Freud and his followers. Then, there is the obvious consideration that conditioning was begun primarily as a physiological rather than a psychological enterprise and that in Russia, contrary to the situation in this country, it continues to be so, particularly when animal subjects or surgery, or both, are involved. The curtailment of Soviet psychology's field of operation, as compared with our own, can thus hardly be a matter of dispute. Imagine an American psychology in which division 5 (Measurement and Evaluation) and most of divisions 8 (Social Psychology) and 12 (Industrial Psychology) were interdicted, a large part of division 3 (Experimental Psychology) was appropriated by some other science, division 12 (Clinical Psychology) was, in general, underdeveloped, and the rest had to conform to a particular school of psychology—let us say, that of Skinner or Hull (together with Marx, Engels, and Lenin, to be sure)—or else!

Yet, within permitted and nonpreempted regions, the actual work of Soviet psychologists is considerable and basic, often reaching beyond the doings and thinkings of the physiologists and not infrequently, I might add, beyond the lines laid down by strict Pavlovian theory. There is, for instance, the very significant experimental area of verbal conditioning, in which psychologists not only participate fully but, by all tokens, seem to be both methodologically and conceptually ahead of physiologists such as Ivanov-Smolnesky, who originally dominated it. Central-psychological methods, such as recognition scores of conditioned stimuli and even imagery reports, are commonly supplementing, under psychologists' tutelage, pure peripheral-physiological techniques of muscle-twitchings, gland-oozings, and electroencephalograms. And results of verbal conditioning experiments are, in their turn, more meaningfully interrelated with ge-

netic studies of development and deterioration of speech and thought. A. R. Luria, who, as you may judge from the 1932 English translation of his book, is quite a sophisticated—and versatile and adjustable—psychologist, is particularly active in the last phase of the area.

Again, there is the area of experimental and theoretical work in perception, which, even without the benefit of Gestalt views—branded as idealistic and reactionary—is being pursued with a good deal of vigor. Perception is related, on the one hand, to orientation (Pavlov's investigatory reflexes) and, on the other hand, to the *Einstellungen* (mental sets) of Ach, Messer, and Külpe. *Einstellungen* (in Russian, *Ustanovski*) have for a long time been regarded with suspicion and even held up to censure, but in the last 2 years they have become respectable through the efforts of the Georgian psychologists, inspired by the late Usnadze (*14*); (Georgia, Stalin's birthplace, seems to have had, in his days, as some readers may know, greater leeway in thought). Then, attention should be called to current Soviet psychologists' experimentation in thinking. At the 1953 All-Union Conference, 12 of the 31 summary reports were in this area, and, interestingly, the experiments reported were quite similar in design and specific interpretations—and in gropings and flounderings—to what goes on in most of our own laboratories, except perhaps for the fact that the Russians use children as standard laboratory subjects. The advantage of having children, rather than rats and dogs and even apes, as subjects in our laboratory experiments has been pointed out before, and the animadversion that our own child psychology studies are too often far removed from hard-core experimentation is not new, and may I add a weak voice of concurrence.

Finally, special mention needs to be made of Soviet applied psychology, which by now is largely applied education psychology dealing with problems of concrete complex learning and training. Since any view that real life in some way transcends laboratory potential would be decried as a heretical remnant of idealism and religion, particularly now when everything transformable in men and animals—in areas that range from apiculture to philosophy and from immunity and ovulation to sports and psychiatry—has been officially declared within the reach of Pavlovian treatments, and since there is only one employer or clientele—the watchful eye of the Soviet State—Soviet applied psychologists differ really but little from their laboratory experimentalists in background, training, and outlook and, unlike our own group, are by no means weak on theory. Pick up a book such as Menchinskayy's *Psychology of Teaching Arithmetic* and you are confronted with an abundance of pedagogical material on the mastery of the subject by school children, but also with a highly sophisticated and detailed discussion of basic learning theory and brain action, the comprehension of which would surely prove difficult to our educational psychologists.

Likewise, examine a symposium on *Problems of the Psychology of Sports* and you discover seven seemingly well-controlled experiments with both large groups of subjects and record-holders in skiing, tennis, track-and-field running, and general gymnastics, together with very well integrated theoretical arguments on the nature of habit formation, perception, and voluntary action. Let me state the main theoretical argument, as presented by the editor of the symposium. Analyzing the data of the seven experiments and quoting Sechenov, who, unlike William James, said that "the more practiced a movement the more it becomes subject to the will," the editor carefully develops the thesis that habits are the most voluntary, and thus the most cognitive, components of man's activities; that habit facility must not be confused with reflex (unconditioned) facility and automation of details with automation of total acts; that assertions about practice telescoping down neural loci are unfounded; and that, in general, the normal dynamics of psychological acquisitions proceed from lower to higher categories and not vice versa. The thesis is obviously not unrelated to the views of the Würzburg school and to Titchener's "meaning core," except that, among other things, the Russian editor rests his arguments largely (but by no means wholly) upon behavioral-objective criteria: the plasticity and controllability of habits, their sensitivity to error, correspondence to objective reality, and so forth.

In short, curtailed as the field of current Soviet psychology is and Procrustean as the beds of its theories are, it seems to manage to forge ahead. Presumably, a psychology with Pavlov in it, even if it has to have also Marx, Engels, and Lenin, is better off, and much better off, than one with Marx, Engels, and Lenin alone. And presumably, while Soviet psychology is experimental only insofar as results do not conflict with basic principles of Soviet philosophy, it is, within that framework, experimental nonetheless and, as such, cannot but correspond in many ways to a respectable portion of our own psychology. And above all there is the happy fact that, in the course of over 50 years, Russia has reared and fostered a special corps of experimentalists—Pavlovian experimentalists—whose special training, skill, and mastery at times display the uniqueness of a Dyaghilev ballet or a Stanislavsky direction and whose methods and findings cannot thus but be of compelling significance to any system of human behavior and any philosophy of human relations. Like a restricted species in a restricted environment, they often burrow in depth, which—to say the least—helps fertilize the common scientific grounds.

Sociopolitical Implications

I have now, in my capacity as a natural scientist, completed what I wanted to say about recent Soviet psychology and psychophysiology, attempting to be as objective and as specific as possible. I would like now to say something that is sociopolitical or sociophilosophical in nature. In popular writing, it is often stated that there is some sort of pact between Pavlovianism and Stalinism—that Stalin and the Stalinists have sought out, selected, and adopted Pavlovian psychology to ruthlessly degrade, enslave, and rape the human mind, for there is some organic relationship between the two. Obviously nothing can be further from the truth. Stalin and Stalinism were not born in 1950, and before 1950—between 1930 and 1950—Pavlovianism as a system of psychology was, as we noted, much less popular in the Soviet Union than in the United States. Stalin did not need Pavlov, nor does Stalinism need him or need any scientific psychology for that matter—and the only reason for choosing Pavlov was his being an eminent Russian in a period when Stalin was morbidly engaged in a glorifying Russification of science and truth. Had Freud and Kohler been Russians, they no doubt would have been the choice.

Indeed, one could say that, from their own standpoint, Stalinists and Marxist-Leninists in general might on the whole have been better off embracing psychoanalysis or even Gestalt psychology. And certainly, and with greater confidence, I am ready to say that Stalin and Stalinists committed the most serious error in their ideological career in proclaiming Pavlovianism as their official psychology and Pavlov as one of their classics. They may have gained experiments on a few hundred dogs but they unwittingly introduced a Trojan horse into their system. For Orwellian-type distortions to the contrary notwithstanding, it is monstrous to deny that the work and views of Pavlov are anything but within the spirit and framework of the best traditions of Western science and democracy. There is not in them, in the work or the views, even an iota of anything specifically Russian or anything that is in the remotest degree related to the credos of Marx or Engels or Lenin, whose names, despite Pavlov's having coexisted for 19 years with the Communist system, do not

even once occur in any of his voluminous published writings, while in a popular unpublished lecture Pavlov sharply criticized Marxism as being dogmatic and unscientific (*15*). The very warp and woof of Pavlovianism stem from a methodology and a philosophy that, in their very essence, are at cross purposes not only with Stalinism but also with aprioristic and absolutistic historical materialism and Marxism in general. And, needless to say, the methodology and philosophy are in line with, and an organic continuation of, the methodologies and philosophies of British empiricists and associationists—Francis Bacon, John Locke, David Hume, John Stuart Mill, and others—whose writings Pavlov studied, and, later, of American animal experimentalists such as Jacques Loeb, Herbert S. Jennings, and Edward L. Thorndike, whom, as many readers may know, Pavlov specifically singled out as being likeminded colleagues in research and thought.

And surely Pavlov was unqualifiedly and unalterably opposed to any curtailment of free inquiry and interpretation. In a letter written in 1914 to the psychologist Chelpanov—a letter the publication of which was until very recently withheld—Pavlov plainly stated that "success in deciphering nature's crowning achievement, the action of the brain, demands absolute freedom, total disavowal of stereotypy, and all possible diversities of points of view and methods" (*16*). And in the afore-mentioned unpublished lecture, delivered in the early '20's, he was even more explicit: "Science and dogmatism are wholly incompatible with each other. Science and free criticism—yes, are synonyms; but dogmatism makes no sense here. Fixed truths? Think of the indivisibility of atoms. In the course of years nothing has remained of it. And science is full of such examples. . . . The dogmatism of Marxism . . . is sheer dogmatism. Because they [the Communists] decided that it is the truth, they don't want to know anything else. . . . If you assume a proper attitude towards science . . . you will realize that Communism and Marxism are by no means absolute truths . . . and then you will look upon life from a free standpoint and not from one of enslavement [in Russian, *zakabalyonny*]" (*17*). Need more be said? And just how does this fit into Stalinism?

Let me conclude with one more idea. The meaning and interpretation of Pavlov and Pavlovianism given here are not unknown and unappreciated in the Soviet Union. As millions of Soviet intellectuals read the writings of Pavlov they cannot but become imbued with a spirit of empiricism and free inquiry and interpretation that is bound to slowly corrode—undermine—the dialectical materialism and thought control of Marx-Engels-Lenin-and-Stalin. One can clearly discern the conflict between the two philosophies in high-level discussions in physiological, psychological, and other technical writings (writings which, I assume, our foreign correspondents and Russian experts are not very likely to be familiar with). And there is in this conflict a strong element of irreversibility: Pavlov and empiricism ascending and dialectical materialism and apriorism descending. The behind-the-curtain political fermentation in the last 3 years is not unrelated to a philosophical-ideological fermentation produced in part, I think, by the penetration of Pavlovian empirical psychology. So that while scientific psychology—and Pavlovian psychology is this, even if we disagree with some of its premises—has not yet, perhaps, cleared the way to a desirable social system, it may well be on the road to changing an undesirable one.

References and Notes

1. Sechenov's original title for the monograph (translated) was *A Physiological Interpretation of Psychological Phenomena*, and he sent it to the literary and sociophilosophical periodical *Sovremennik*. The censor, however, refused permission for publication, whereupon Sechenov submitted the monograph to the *Medical Courier* and changed the title to what it is now, *Reflexes of the Brain*.
2. N. I. Bukharin, *Ataka* (GIZ, Moscow, 1924), p. 127; p 171. Bukharin's two articles are entitled, respectively, "Yenchmeniada [after Yenchmen, a radical Soviet behaviorist], the Problem of an Ideological Degeneration" and "World Revolution, Our Country, Culture, etc. (a Reply to Professor Pavlov)." The "reply" in the second article is made to an unpublished lecture delivered by Pavlov. A few salient sentences of the lecture, as quoted by Bukharin, are given at the end of this article.
3. L. S. Vygotsky, in *Psikhologiya Marksizma* (GIZ, Moscow, 1925); Y. V. Frankfurt, *Reflexology and Marxism* (GIZ, Moscow, 1926); Z. I. Chuchmarev, *Subcortical Psychophysiology* (GIZ, Kharkov, 1928); K. N. Kornilov and Y. V. Frankfurt, *Vestnik Kommun. Akad.* 181, 35 (1929).
4. S. L. Rubinstein, *Principles of General Psychology* (UCHPEDGIZ, Moscow, 1940).
5. *Scientific Sessions on the Problems of the Physiological Teachings of Academician I. P. Pavlov. June 28–July 4, 1950* (Akad. Nauk S.S.S.R., Moscow, 1950).
6. P. A. Rudik, *Psikhologiya* (UCHPEDGIZ, Moscow, 1955); A. V. Zaporozhets, *Psikhologiya* (UCHPEDGIZ, Moscow, 1955); V. A. Artyomov, *Ocherk* [Outline] *Psikhologii* (UCHPEDGIZ, Moscow, 1954); P. I. Ivanov, *Psikhologiya* (UCHPEDGIZ, Moscow, 1954); B. M. Teplov, *Psikhologiya* (UCHPEDGIZ, Moscow, 1954); T. G. Yegorov, *Psikhologiya* (Ministry of Defence, Moscow, 1954); K. N. Kornilov, A. A. Smirnov, B. M. Teplov, Eds., *Psikhologiya* (UCHPEDGIZ, Moscow, 1948).
7. G. P. Zelyoniy, *Compt. rend. soc. biol.* 75, 659 (1913).
8. P. K. Anokhin, *Problems of Higher Nervous Activity* (Akad. Nauk, S.S.S.R., Moscow, 1949).
9. ———, *Fiziol. Zhur. S.S.S.R.* 38, 758 (1952).
10. D. O. Hebb, *The Organization of Behavior* (Wiley, New York, 1949).
11. I. P. Pavlov, *Collected Works* (Akad. Nauk, S.S.S.R., Moscow, 1951), vol. 3, pt. 2, p. 325.
12. ———, *Pavlov's Wednesday Seminars* (Akad. Nauk, S.S.S.R., Moscow, 1949), vol. 3.
13. V. I. Kryazhev, *Sovet Nevropatol. Psikhiat. i Psikhogig.* 1, 778 (1932).
14. *Voprosy Psikhol.* 1, 72 (1955).
15. Quoted by Bukharin (*2*).
16. *Voprosy Psikhol.* 1, 99 (1955).
17. As quoted by Bukharin, (*2*). Bukharin identifies the quotations as coming from the stenographic report of a public lecture. And it should be noted that whereas all Pavlov's known manuscripts and letters and even students' notes of his seminars were published posthumously (five volumes of seminar notes have appeared so far), not only has this lecture been left unpublished, but its very existence is nowhere mentioned in any of the several Pavlov biographies, bibliographies, and published chronological lists of his public and professional activities.

News of Science

Hunters or Hunted?

During recent years, Raymond A. Dart, to whom belongs the credit of having discovered the first of the Australopithecines, has written at considerable length about the social life of these interesting and controversial "man-apes" from the early Pleistocene of South Africa. In various papers, from 1948 onward, he has pictured these creatures as fire-users, hunters, and bone-collectors (more specifically, head-hunters), and as possessing a primitive bone-tooth-horn culture. One must perforce admire the ingenuity and imagination displayed by Dart in his reconstruction of australopithecine society. Yet the data upon which his deductions are based, and hence his conclusions themselves, have proved somewhat short of convincing to at least some students of human evolution.

The evidence advanced by Dart [*Am. J. Phys. Anthropol.* N.S. **6**, 259 (1948), *et seq.*] for the deliberate use of fire by these creatures has not withstood critical analysis [Oakley, in *An Appraisal of Anthropology Today*, Tax *et al.*, Eds. (Univ. of Chicago Press, 1953), pp. 29–31; *Weekly Evening Meeting Roy. Inst. Gr. Brit.* (20 Nov. 1953); *Am. J. Phys. Anthropol.* N.S. **12**, 9 (1954); also see comments by Straus, *Science* **120**, 356 (1954)]. Moreover, competent students, such as Oakley [*Weekly Evening Meeting Roy. Inst. Gr. Brit.* (20 Nov. 1953)] and Von Koenigswald [*Proc. Koninkl. Ned. Akad. Wetenschap. Ser. B* **56**, 403 (1953)] have ascribed the accumulations of nonaustralopithecine bones found in the australopithecine deposits to the activities of carnivores, including hyenas. Yet Dart and his pupil Hughes have persisted in their attempts to dismiss the bone-accumulating hyena as a myth [Hughes, *Am. J. Phys. Anthropol.* N.S. **12**, 467 (1954); Dart, *Ann. Rept. Smithsonian Inst. for 1955* (Washington, D.C., 1956), p. 317; Dart, *Am. Anthropologist* **58**, 40 (1956)] and to attribute these amassments to deliberate, selective, collecting proclivities of the "man-apes"—this despite well-attested evidence of bone-accumulation by cave-dwelling hyenas in the Pleistocene [Zapfe, *Forsch. u. Fortschr.* **15**, 269 (1939); *Z. Gesamtgebiet Geol. u. Mineral. sowie d. Angewand. Geophys.* Suppl. **12**, 1 (1954)]. Indeed, Dart [*Ann. Rept. Smithsonian Inst. for 1955* (Washington, D.C., 1956)], with more fervor than persuasiveness, has conjured up an "osteodontokeratic culture" for the Australopithecines from these piles of bones and has reconstructed therefrom his notions of the hunting techniques of these animals. To equate the supposed weapons of the Australopithecines with those of Hercules and Samson may appeal to one who is convinced that these Pleistocene primates are his lineal ancestors; but it is scarcely justifiable.

Just as Oakley (*vide supra*) demolished the supposed proofs of australopithecine pyrotechny, Washburn [*Am. Anthropologist* **59**, 612 (Aug. 1957)] recently has presented evidence which seriously questions the reality of the so-called "osteodontokeratic culture" of the "man-apes." While looking for baboons in the Wankie Game Reserve, Southern Rhodesia, in 1955, Washburn made systematic records of the bones found in 35 recent kills of carnivores. The distribution of bones in these kills (skulls and vertebrae preponderating) caused him to conclude that the high frequency with which nonaustralopithecine skulls, jaws, and upper cervical vertebrae occur in australopithecine deposits does not necessarily constitute evidence that the "man-apes" were hunters; rather, it may be a consequence of selective eating by carnivores, for such a type of bone collection is the result of the normal eating habits of these animals. Although he emphasizes that a variety of animals may have been involved as agents in the production of these accumulations of fossil bones, Washburn is strongly inclined to the belief that hyenas were an important factor. He notes that the brown hyenas of Kruger National Park collect the heads of medium-sized antelopes, baboons, and a few carnivores. Not only are these the kinds and distribution of bones found in australopithecine deposits, but in addition, hyena coprolites have been found therein. Washburn thus concludes that it is "probable that the australopithecines were themselves the game, rather than the hunters."

Dart's "osteodontokeratic culture" hitherto was, at best, *sub judice*. Washburn's dispassionate and objective study now makes its actuality even somewhat less than improbable.

WILLIAM L. STRAUS, JR.
Johns Hopkins University

NSF Support for Life Sciences

The Division of Biological and Medical Sciences of the National Science Foundation has announced that the next closing date for receipt of research proposals in the life sciences is *15 January 1958*. Proposals received prior to that date will be reviewed at the winter meetings of the foundation's advisory panels, and disposition will be made approximately 4 months after the closing date.

In addition to funds for the support of basic research in the life sciences, limited funds will be available during the current fiscal year for the support of research facilities and programs at biological field stations. Inquiries should be addressed to National Science Foundation, Washington 25, D.C.

AEC Office in Tokyo

An Atomic Energy Commission office was opened in Tokyo, Japan, on 15 November. W. H. Pennington has been appointed scientific representative to head the office, with P. A. Roessler as assistant.

The Tokyo office will aid in the scientific and technical aspects of the rapidly expanding atomic energy developments in Japan. The new unit's activities will include liaison with the Japanese atomic energy authorities and scientists in connection with implementation of the bilateral agreement for cooperation in the uses of atomic energy. The AEC scientific representative will also assist the Department of State, the International Cooperation Administration, and the U.S. Information Agency.

Engineering Salaries

The average engineering teacher in American colleges and universities earns a salary of $6634 per year. He adds consulting and other engineering work to bring his total annual earnings to $8862. He earns more if he teaches in a privately endowed institution than he does if he teaches in a public institution, and he earns more on the Pacific Coast than anywhere else in the nation.

These figures come from a new survey by the American Society for Engineering Education of 1956 engineering salary figures originally gathered by the Engineers Joint Council. The A.S.E.E.'s

analysis was made in connection with a high-priority study of how to increase the supply of competent teachers for engineering colleges.

The survey found that average salaries for young engineers serving as instructors range from $4214 in public institutions to $4374 in privately supported schools. Department heads' salaries average $9117 and $9839, respectively. Deans earn $10,932 and $11,829, respectively, on the national average.

Of the 4000 engineering teachers surveyed, 80 percent earned income beyond their teaching salaries from the practice of engineering. Instructors earned an average of over $1000, professors from $2436 (public institutions) to $4716 (private institutions). On the average, teachers in public institutions had outside earnings of $2333, those in private institutions $3634.

Engineering teachers' salaries do not vary widely across the nation. Averages in public institutions range from a low of $4086 in the Mountain states to $6886 in the Midwest, in private institutions from $5133 in the Mountain states to $7613 on the Pacific Coast. But average annual incomes vary more, from a low of $7319 in the Mountain states to $12,500 in the Pacific states, and there is a substantial spread around the national average.

The figures released by the A.S.E.E. were based on replies from more than 4200 engineering teachers, 35 percent of the 12,000 engineers engaged in teaching in the United States. Revised salary data will be presented in a final report, together with recommendations of the Committee on the Development of Engineering Faculties. This is scheduled for release early in 1958. The A.S.E.E. project was made possible by grants from the National Science Foundation and from private and industrial organizations.

Books for Asian Students

In the past 2½ years the Asia Foundation's special project, Books For Asian Students, has sent 600,000 books to more than 1200 universities, colleges, libraries, and civic groups in Asia. These books were donated by 700 university and college groups, publishers, libraries, and individuals in the United States. The great need for books continues, as evidenced by increasing requests.

Contributions of books will be greatly appreciated. Items in every category on the university and college level, in good condition, published in 1948 or after, and works by standard authors, regardless of date, can be sent directly to: Books for Asian Students, 21 Drumm St., San Francisco 11, Calif. The foundation will reimburse donors for the transportation costs of substantial shipments to San Francisco. All contributions are tax exempt.

The Asia Foundation, a nonprofit, nonpolitical organization founded by private American citizens, supports individuals and groups in Asia who are working for the "attainment of peace, independence, personal liberty and social progress." The foundation maintains 18 offices in Asia.

Rockefeller Foundation Grants

Rockefeller Foundation grants during the third quarter of 1957 totaled $1,890,525. Grants in the field of medical education and public health amounted to $304,915; biological and medical research, $350,650; agriculture, $384,240; social sciences, $186,415; and humanities, $251,105; general appropriations totaled $413,200.

During the same period 123 fellowships that had been awarded to individuals from 30 countries and one international organization became active.

Ford Foundation Awards

The Ford Foundation recently announced grants and appropriations totaling $49,187,371 in the final quarter (July through September) of its 1957 fiscal year. The total includes $25.6 million in grants out of appropriations announced in previous quarters. Of this amount, a $24.5 million appropriation approved in March was granted during the final quarter to the Woodrow Wilson fellowship program to attract outstanding students to college teaching careers.

During the quarter the foundation completed its program in support of training and research in the behavioral sciences and mental health with grants totaling $9,819,150 to colleges, universities, and research centers. The largest award was a $5-million grant for the continued operation of the Center for Advanced Study in the Behavioral Sciences, Stanford, Calif., until August 1964.

UN Radiation Committee

The Scientific Committee on the Effects of Atomic Radiation, established by the United Nations General Assembly in December 1955, will hold its fourth session beginning 27 January 1958. The announcement of the date and agenda for the session has been sent to the committee's 15 members: Argentina, Australia, Belgium, Brazil, Canada, Czechoslovakia, Egypt, France, India, Japan, Mexico, Sweden, the U.S.S.R., the United Kingdom, and the United States.

When the committee was set up, it was asked among other things to collect all available information on the effects of radiation on man and his environment, and to develop by mid-1958 a summary and evaluation of the reports received. The first draft of this comprehensive report will be discussed at the January session.

A yearly progress report to the General Assembly that was distributed recently notes that the committee met twice in 1956 and again in April 1957. Zenon Bacq of Belgium is chairman, and E. A. Watkinson of Canada is vice-chairman. As part of its work of gathering information, the Scientific Committee so far has received 130 reports from 27 governments and U.N. specialized agencies.

Federal Research Budget

In fiscal year 1957, Federal Government expenditures for scientific research and development amounted to around $3 billion, according to a report released by the National Science Foundation. The $3-billion expenditure reflects an increase of almost 20 percent over the 1956 expenditure of $2.5 billion. The estimate is included in *Federal Funds for Science VI*, the latest in an NSF series of surveys of the Federal Government's research and development budget. The NSF report includes an analysis of the Government's financial obligations in terms of administering agencies, character of work, scientific fields, and organizations performing the work.

In fiscal year 1957, the Department of Defense and the Atomic Energy Commission accounted for 85 percent of the funds. Along with these two agencies, six other agencies—the Department of Health, Education, and Welfare, the Department of Agriculture, the National Advisory Committee for Aeronautics, the Department of the Interior, the National Science Foundation, and the Department of Commerce—were responsible for all but 1 percent of the research and development budget.

More than 60 cents of every dollar for conducting research and development was obligated for development; less than 40 cents for research, both applied and basic. Basic research accounted for 8 cents.

Of the $964 million obligated for basic and applied research in fiscal year 1957, the physical sciences, including engineering, claimed 67 percent; the life sciences, 29 percent; and the social sciences, 4 percent.

Thirty-five cents of each Federal dollar for basic and applied research and development went to profit organizations, 14 cents went to educational insti-

tutions, 48 cents remained within the Federal Government, and 3 cents went to "other" institutions. Copies of *Federal Funds for Science VI* may be obtained from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C., for 35 cents.

News Briefs

Francis Perrin, high commissioner for Atomic Energy of France, has been selected as president of the Second International Conference on the Peaceful Uses of Atomic Energy to be held 1–13 September 1958 in Geneva. The selection was made at a recent meeting of the United Nations Advisory Committee on the Peaceful Uses of Atomic Energy. The president of the first Conference on the Peaceful Uses of Atomic Energy, which met in Geneva in August 1955, was Homi Bhabha of India.

* * *

The Science Council of Japan adopted at its 25th general meeting on 4 October a "Resolution in Support of the Statement of the International Meeting of Scientists at Pugwash" [*Science* **125**, 199 (2 Aug. 1957)].

* * *

The School of Mathematics of the Institute for Advanced Study, Princeton, N.J., will allocate a small number of grants-in-aid to gifted young mathematicians and theoretical physicists to enable them to study and conduct research at Princeton during 1958–59. Candidates must have given evidence of ability in research comparable at least with that expected for the Ph.D. degree. Application blanks may be obtained from the School of Mathematics; they must be returned by *1 January 1958.*

* * *

The third atoms-for-peace mission to the American republics arrived in Managua, Nicaragua, on 9 November for a 3-week trip through Nicaragua, El Salvador, Guatemala, and Honduras. The seven-man U.S. group met with scientists, educators, and government officials to exchange ideas on practical applications of nuclear energy. Adaptation of existing educational facilities to provide specialized training, and possible U.S. cooperation in this effort, was on the agenda for discussion with each country.

* * *

A new type of written science test is being devised by the College Entrance Examination Board to find true potential scientists as opposed to "gadgeteers." The plans for the test were described at a recent conference of the New York Association of Biology Teachers.

* * *

Carpenter Hall, new library and administrative center for the College of Engineering at Cornell University, was dedicated recently by the donor, Walter S. Carpenter, Jr., chairman of the board of E. I. du Pont de Nemours & Co.

* * *

The Health Information Foundation has announced that in 1958 it will sponsor and jointly conduct with the National Opinion Research Center of the University of Chicago another nationwide survey of medical costs and voluntary health insurance coverage. The new study will be a resurvey of the HIP-NORC study of 1953, which provided basic health insurance data.

* * *

A dinosaur egg has been presented to the Harvard University Museum of Comparative Zoology by the city of Aix-en-Provence, France, The 20-pound egg is one of a new find of whole eggs of a hypsilosaurus, one of the large dinosaurs. Until the discovery at Aix-en-Provence by Raymond Dughi, only whole eggs of one small dinosaur had been available for study. These Aix-en-Provence dinosaur eggs are the largest reptile eggs known to science.

* * *

The Franklin Institute Museum, Philadelphia, Pa., has planned a new two-floor basic chemistry exhibit to be opened early in 1958. It is estimated that the exhibit will cost in the neighborhood of $100,000 to build and another $70,000 to operate for the next 5 years. Supporting funds are being solicited from industry.

December Scientific Monthly

Articles appearing in the December issue of *The Scientific Monthly* are: "Outlook to Space Travel," E. Stuhlinger; "Scientist in Contemporary Fiction," B. Glass; "The Last Geographic Frontier: the Sea Floor," E. L. Hamilton; "Scientific Progress and Political Science," B. Brodie; "Course Requirements for Future Science Teachers," F. G. Watson. Thirteen books are reviewed.

Scientists in the News

GARTH J. THOMAS has been appointed research professor jointly in the Bioacoustics Laboratory and the department of physiology, University of Illinois, Urbana. His research will be concerned with studies of behavioral effects of brain lesions produced by focused ultrasonic energy.

HANS A. BETHE of Cornell University recently completed a series of four Morris Loeb Lectures in physics at Harvard University. He discussed "The Nuclear Many-Body Problems."

WALTER M. BEJUKI, formerly research associate at the Prevention of Deterioration Center, Division of Chemistry and Chemical Technology, National Academy of Sciences–National Research Council, has been appointed assistant director of the center.

JAMES B. McNAUGHT, professor and head of pathology at the University of Colorado Medical Center, Denver, has received the Certificate of Highest Merit and a gold medallion "in recognition of contributions made to the science of Clinical Pathology and to the American Society of Clinical Pathologists." The presentation was made during the joint annual meeting of the American Society of Clinical Pathologists and the College of American Pathologists that took place recently in New Orleans, La.

PAUL K. KURODA, formerly associate professor of chemistry at the University of Arkansas, has joined the staff of Argonne National Laboratory as associate chemist. Kuroda, who is the only naturalized citizen of Japanese birth on the Argonne staff, will be especially concerned with problems of low-level radioactivity.

GEORGE B. KOELLE, professor of pharmacology in the department of physiology and pharmacology in the University of Pennsylvania's Graduate School of Medicine, has been elected dean of that school. He succeeds GEORGE M. PIERSOL, who has served the university for 50 years as a teacher and administrator in the field of medical education.

Coincident with Koelle's election, which became effective on 1 Nov., it was announced that a revised curriculum will be instituted in the Graduate School of Medicine in September 1958. The new curriculum, formulated by a faculty committee of which Koelle was chairman, is designed to meet changing requirements for medical specialty training both in this country and abroad. At present, the Graduate School of Medicine conducts an 8-month program in which study of the basic medical sciences is carried on concurrently with clinical training in 22 specialties.

In place of this combined program, the new and more flexible curriculum to be established next year provides for two separate semesters of 4 months each. All the basic medical sciences, as well as those sciences that a physician requires for his specialty, will be offered during the first semester. The second semester will be devoted entirely to clinical training.

ALBERT C. ZETTLEMOYER, professor of chemistry and director of the

National Printing Ink Research Institute at Lehigh University, has received the Mattiello Award of the Federation of Paint and Varnish Production Clubs, the highest award conferred by the paint industry.

BERNARD I. SPINRAD, associate director of the Reactor Engineering Division at Argonne National Laboratory, has succeeded the late Arthur H. Barnes as director of the division. Spinrad received his doctor's degree in physical chemistry from Yale University in 1945 at the age of 21.

JOHN McK. MITCHELL, dean of the School of Medicine at the University of Pennsylvania since 1948, has been appointed presiding dean of the university's two medical schools. Mitchell will coordinate the programs of the School of Medicine, which he continues to serve as administrative head, and the Graduate School of Medicine, of which George B. Koelle is dean. In addition, Mitchell will be executive officer for the newly created Joint Medical Council. The council, with I. S. Ravdin as chairman, will advise Mitchell on educational policy and procedures and will help to integrate more closely the work of the university's medical schools and hospitals.

DAVID GOULD, senior research chemist at Schering Corporation, Bloomfield, N.J., has been appointed to the newly created position of administrator, extramural scientific research.

WERNHER VON BRAUN, head of a research group at Redstone Arsenal, Huntsville, Ala., and former rocket specialist for Nazi Germany, has received the Army's Exceptional Civilian Service Award for his part in creation of the Jupiter, an intermediate-range ballistic missile.

LOWELL J. REED, president emeritus of Johns Hopkins University, has received the Sedgwick Memorial Medal of the American Public Health Association "for distinguished service in public health." Reed retired from Johns Hopkins last year after serving on the faculty since the establishment of the School of Hygiene and Public Health in 1918.

JOHN E. BLAIR, bacteriologist at the Hospital for Joint Diseases, New York City, has received the 1957 Kimble Methodology Research Award, one of the nation's outstanding honors in the field of public health. Blair was the first in this country to adopt the British technique known as bacteriophage typing and develop it for use in American laboratories. He became the principal source of supply in the United States for the pure strains of bacteriophage now being used for typing in laboratories throughout the nation. His laboratory at the Hospital for Joint Diseases has been designated as the nation's central reference laboratory for this work.

By using Blair's adaptation of bacteriophage typing, investigators can, for example, determine exactly who, or what, is responsible for an outbreak of food poisoning. By typing the precise strain of staphylococcus causing the trouble, it is possible to break through the large number of other strains that are often present and trace the staphylococcus back to its source, often a human carrier. Bacteriophage typing can also tell a physician whether or not the staphylococcus causing a disease or a wound infection is one of those strains which are frequently resistant to some kinds of antibiotic drugs.

HANS E. HOLIMANN, former German scientist and now a consultant physicist in Los Angeles, Calif., has received the honorary degree of doctor of engineering from the Technical University of Dresden, Germany.

The following emeriti professors have begun a year of teaching in independent, liberal arts colleges under the Whitney Visiting Professors Program.

M. C. ELMER, professor of sociology, University of Pittsburgh, Pittsburgh, Pa., at Western College for Women, Oxford, Ohio.

HORNELL HART, professor of sociology, Duke University, Durham, N.C., at Centre College of Kentucky, Danville.

WILLIAM E. SHIDELER, Miami University, Oxford, Ohio, at Hiram College, Hiram, Ohio.

ORRIN H. SMITH, professor of physics, DePauw University, Greencastle, Ind., at Kalamazoo College, Kalamazoo, Mich.

HARVEY STORK, professor of botany, Carleton College, Northfield, Minn., at Tougaloo Southern Christian College, Tougaloo, Miss.

The Whitney Visiting Professors Program was established in 1952 to serve a twofold purpose: (i) to honor individuals who have not only distinguished themselves by a lifetime of classroom teaching but who have also retained the physical and mental vigor to continue their important contribution to American youth, and (ii) to strengthen teaching in independent, liberal arts colleges throughout the country.

M. G. CANDAU, director-general of the World Health Organization, has accepted the offer made last spring by the 10th World Health Assembly to renew his contract to head WHO for a second term. Candau has asked that the renewal be made for 2 years, starting 21 July 1958, when his present term of office expires.

JOHN D. ROBERTS, professor of organic chemistry at California Institute of Technology, delivered the 13th annual Harrison Howe Lecture, which is sponsored by the Rochester Section of the American Chemical Society.

Recent Deaths

MIGUEL A. CATALAN; Madrid, Spain; 63; professor of atomic structure at Madrid University; in 1921 discovered the methods of multiplets used universally in nuclear research ever since; 11 November.

JOHN J. GILBERT, Rye, N.H.; 70; ocean cable engineer with the Simplex Wire Cable Company at Newington, N.H.; retired in 1954 from the Bell Laboratories of the American Telephone and Telegraph Company, where he helped to develop the Key West–Havana telephone cable; held 50 patents relating to cable technology; 11 November.

ROBERT T. KNAPP, Los Angeles, Cal.; 58; professor of hydraulic engineering at California Institute of Technology; 7 November.

ARNO B. LUCKHARDT, Chicago, Ill.; 72; distinguished service professor emeritus of physiology at the University of Chicago; discovered the anesthetic properties of ethylene gas in 1923; 7 November.

GEORGE W. MERCK, West Orange, N.J.; 63; chairman of Merck & Co., Inc.; during World War II a special consultant on biological warfare to the Secretary of War and in 1944 became chairman of United States Biological Warfare Committee; 10 November.

HENRY MOUQUIN, Sparkill, N.Y.; 60; former associate professor of chemistry at New York University; 13 November.

GILBERT RATHMAN, Union, N.J.; 68; mechanical engineer and inventor; consultant to Roots-Connersville Blower, division of Dresser Industries, Inc., which developed his patented blower; 12 November.

JOSEPH A. STEPHAN, Cincinnati, Ohio; 77; general superintendent of the Cincinnati zoo from 1937 to 1949; past-president of the American Association of Zoological Parks; 6 November.

HENRY A. STRAUS, Lexington, Mass.; 43; physicist at the Lincoln Laboratory of Massachusetts Institute of Technology; formerly principal engineer for the Bendix Radio Corp.; made important contributions to mass spectroscopy and development of fire-control radar; 21 September.

Reports

Observation of Bioluminescence in the Atlantic Fish (Porichthys porosissimum)

The only North American shallow-water fishes which produce light belong to the genus *Porichthys* (family Batrachoididae). The fact that the Pacific species, *P. notatus,* will produce light is well known.

Greene (*1*) observed that *P. notatus,* when it was placed in an aquarium made alkaline with ammonia water, "exhibited a brilliant white light for about 20 minutes." Dean (*2*) stated that there were "few actual observations of living fishes." Greene and Greene (*3*) reported that *P. notatus* had a latent period of 8 to 10 seconds following stimulation and that the light lasted about 20 seconds. They also reported that adrenalin, when it was injected into the fish, activated the light-forming organs. Prosser *et al.* (*4*) stated that intermittent light of this type "is an intracellular phenomenon," but the exact mechanism is still not known. Hubbs and Schultz (*5*) gave a bibliography on this group of fishes. Harvey (*6*) states that little work additional to that of Greene and Greene has been done "chiefly due to lack of material."

There seem to be no reports of observations on light production in the Atlantic midshipman, *P. porosissimum.* Jordan and Evermann (*7*) reported that this species had been seen by fishermen "shining at night" but that they had been unable to verify the observation. The following observations were made at the Gulf Coast Research Laboratory on the night of 22 June 1957. The fish came from Mississippi Sound.

At 9 P.M., after the lights had been off for about 10 minutes, a faint glow appeared to move toward the water surface. Before the lights were turned off, one of the two midshipmen present had been cruising up and down and around the tank. There were several more short emissions of light lasting 5 to 10 seconds, but interrupted by variously longer periods of dark. One show of light was so intense that the rows of photophores on the ventral side, the row above the middle of the side, and the several rows on head and chest stood out sharply as bright lights for 15 or 20 seconds. After one or two faint emissions, nothing more could be seen even though the fish was picked up in the hand and released. The following night the aquarium was again observed for more than an hour; the fish was stimulated with the hand, but no light was seen. Except for the removal of one *Gobionellus hastatus* from the tank, conditions were essentially the same as they had been on the previous night. There was about 1 inch of white sand in the aquarium, which was 10 by 18 inches by 12 inches high. The aquarium contained two *Porichthys,* one *Hippocampus hudsonius,* and two shrimp (*Penaeus*). The salinity was 16.9 parts per thousand.

On 25 June a midshipman was placed in a liter of sea water to which about 5 ml of ammonia water had been added. The same photophores that showed brightly on the one spontaneous emission became bright to about the same intensity. The fish became very active during this time and was killed by the ammonia. After respiratory and other movements ceased, the light slowly diminished.

On 29 July tests were made to see whether other chemicals would stimulate light production. These chemicals were added slowly, a few milliliters at a time, to a gallon jar containing the fish in 1 liter of sea water which had a salinity of 26.2 parts per thousand and *p*H of 7.2.

Sodium hydroxide and ethyl alcohol were as effective as ammonia, but acetic acid failed to stimulate any visible light even though it was added until the fish had died. Light did not appear in the alcohol test until 50 ml of 95-percent alcohol had been added and the fish had lost almost all ability to move. The light continued to increase as alcohol up to 112 ml was added. In the NaOH test, maximum luminescence was reached after 73 ml of 1N solution of the alkali had been added and the *p*H had become 11.2.

Porichthys porosissimum is distributed from South Carolina to Uruguay and is found in shallow waters. It is not a particularly abundant fish, nor is it extremely rare. The only estimates of its general abundance were given by Gunter (*8*), who collected eight specimens from April to November 1941 among 78,000 fishes which were taken mostly by trawl from Texas bays. He found the species at salinities ranging from 10.3 to 35.8 parts per thousand. In Mississippi Sound the fish seems to be more abundant, and one to ten are taken in an hour's trawling. The fish is not at all delicate and exhibits the well-known toughness of the batrachoidids. It is a readily available source for workers studying the physiology of bioluminescence.

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5 August 1957

Tumor-Inhibiting Effects Derived from an Active Principle of Garlic (Allium sativum)

Extracts of garlic (*Allium sativum*) have been shown to contain a powerful bactericidal agent, allylthiosulfinic allyl ester (allicin) (*1, 2*). This compound is formed by the interaction of an enzyme and substrate present in garlic bulbs (*3*). The enzyme, alliinase, is liberated when the garlic bulb is crushed, and it acts on the substrate, S-allyl L-cysteine sulfoxide (alliin) as follows (*4*):

$$2\,R\text{—SO—CH}_2\text{—CH(NH}_2\text{)—COOH} \quad \text{(alliin)}$$

$$\xrightarrow[\text{alliinase}]{+\,H_2O} R\text{—SO—S—}R + \quad \text{(allicin)}$$

$$2\text{CH}_3\text{—CO—COOH} + 2\text{NH}_3$$

where

$$R \text{ is } \text{—CH}_2\text{—CH=CH}_2.$$

Wills (*5*) has shown that this reaction product inhibits many sulfhydryl (—SH) enzymes but that it does not affect many

All technical papers and comments on them are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table or one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* 125, 16 (4 Jan. 1957).

other enzymes. There is a close relationship between the structure required for bactericidal action and that required for inactivation of —SH enzymes. Thus compounds containing the —SO—S— grouping are effective in both enzyme inhibition and bactericidal activity, whereas compounds containing the —SO—, —S—S— or —S— linkages are ineffective. The —SH inactivation obtained with alkylthiosulfinic alkyl ester may be the result of a strong combination of this compound with cysteine or may be due to oxidation of —SH to —S—S— by the labile oxygen (*2, 5*).

Although most of the studies of allicin have centered upon its bactericidal action, its reactivity with —SH groups suggests that it might also have an inhibitory effect on malignant cells. An increase in —SH compounds prior to cell division has been demonstrated in a wide variety of tissues, plants, and organisms (*6*). Reduced —SH compounds stimulate cell growth and division, whereas substances which oxidize —SH to —S—S— inhibit cell division. Similar inhibition of cell division may be obtained with thiol poisons such as alkylating agents and heavy metals. Abnormalities of —SH metabolism may be implicated in malignant cell growth since a high —SH content has been demonstrated in some tumor cells (*7*). Accordingly, the effect of an alkylthiosulfinic alkyl ester on the growth of malignant tumors in animals was studied.

Since the allyl ester of allylthiosulfinic acid which is ordinarily formed in garlic extracts is unstable, the more stable diethyl analog was used in these studies. The ethylthiosulfinic ethyl ester (C_2H_5—SO—S—C_2H_5, ETHIOS) was prepared by incubating S-ethyl L-cysteine sulfoxide with alliinase. Alliinase was prepared from crushed garlic bulbs according to the modification of Wills (*5*). S-Ethyl L-cysteine sulfoxide was prepared from S-ethyl L-cysteine by oxidation with 30 percent hydrogen peroxide and crystallization from aqueous acetone. The amount of thiosulfinic ester formed was estimated by determining the amount of ammonia released from the reaction mixture after incubation with alkali in a Conway vessel.

Studies were made of the effect of these substances on the growth of sarcoma 180 ascites tumor in CFW Swiss mice (18 to 22 g). Gain in weight and time of survival were taken as an index of the number of ascites tumors formed and of the degree of malignancy produced. The mice were inoculated with a dilute suspension of tumor cells freshly drawn from donor mice. Each inoculum contained approximately 5 million tumor cells and was incubated with either normal saline or an equivalent volume of the test substance in solution for 10 to 15 minutes prior to intraperitoneal inoculation. Incubation with saline prior to inoculation was uniformly followed by successful transplantation in each of 75 control mice, as is shown by rapid weight gain and death within 16 days (Fig. 1 and Table 1).

Table 1. Effect of garlic enzyme (alliinase) and substrate (S-ethyl L-cysteine sulfoxide) on the development of sarcoma 180 ascites tumor in mice. The synthetic reaction product was ethyl thiosulfinic ethyl ester (C_2H_5—SO—S—C_2H_5; ETHIOS).

Inoculum	No. of animals	Percentage developing tumors	Survival (day)
Tumor + saline	75	100	< 16
Tumor + substrate	25	100	< 16
Tumor + enzyme	25	100	< 16
Tumor + (substrate + enzyme)	25	0	> 180
Tumor + synthetic reaction product	50	0	> 120

Similar results were obtained when the inoculum was preincubated with either the enzyme (alliinase) or the substrate (S-ethyl L-cysteine sulfoxide). However, when the tumor cells were preincubated with equivalent amounts of a solution in which the enzyme and substrate had been allowed to react, no growth of the ascites tumor was demonstrable, and there was no mortality in animals that were observed for a period of 6 months. Approximately 1.0 μmole of the enzymatically prepared ethylthiosulfinic ethyl ester was present in each inoculum. Heating the enzyme before allowing it to react with the substrate resulted in complete failure to inhibit formation of ascites tumors, the gain in weight and the mortality being identical with those obtained in the control animals.

Preincubation of tumor cells with the ethylthiosulfinic ethyl ester synthetically prepared by oxidation of diethyl disulfide with perbenzoic acid (*8*) was also effective in preventing tumor growth. Successful inhibition was obtained with 0.1 μmole of the synthetically prepared ester per inoculum. Iodoacetate, which also inhibits some thiols, did not prevent formation of ascites tumors or death when preincubation was carried out with 1 μmole per inoculum.

Intravenous injections of the ethylthiosulfinic ethyl ester into mice previously inoculated with sarcoma 180 ascites tumor cells delayed the onset of malignant ascites tumors and in some instances completely prevented their formation and the death of the mice. Five micromoles of the ester were injected intravenously into mice daily for 7 days, the first injection being given 24 hours after intraperitoneal inoculation with sarcoma 180 ascites tumor cells. When the inoculum was 5 million ascites cells per animal, no ascites tumors developed during this period of time, but malignant ascites tumors developed rapidly after intravenous administration of the ester was discontinued. When the tumor inoculum was decreased to 1 million cells per animal, 40 percent of the animals failed to develop ascites tumors even when intravenous injection of the ester was discontinued.

The effect of the ethylthiosulfinic ethyl ester on incorporation of a radioactive sulphur (S^{35}) amino acid by leukemic leukocytes *in vitro* was also studied. Leukemic leukocytes were incubated with the thiosulfinic ester for 30 minutes; the cells were then centrifuged and washed, and S^{35} L-cysteine was added. Following incubation, the uptake of S^{35} was determined by methods previously described (*9*). Addition of increasing amounts of either the enzymatically formed or synthetic thiosulfinic ester resulted in a progressive decrease in the uptake of S^{35}. With 2.5 μmole of the ester per milliliter of whole blood, the uptake of S^{35} was reduced to 10 percent of the control values.

It is evident that ethylthiosulfinic ethyl ester may have tumor-inhibiting effects when malignant cells are placed directly in contact with this compound prior to inoculation. The inhibitory effect of the thiosulfinic ester on tumor growth when not placed directly in contact with this compound is suggestive but not conclu-

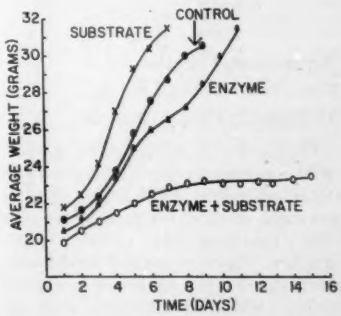

Fig. 1. Effect of garlic enzyme and substrate on growth of sarcoma 180 ascites tumor in mice. Preincubation of the inoculum with saline (control) results in rapid growth and death of all animals within 16 days. Preincubation with the enzyme (alliinase) or substrate (S-ethyl L-cysteine sulfoxide) results in a similar rapid growth of the tumor and death of all the animals within 16 days. When the enzyme was allowed to react with the substrate, and the inoculum was preincubated with the reaction mixture, no tumor growth occurred, and the animals remained alive during a 6-month observation period.

sive. Both the bactericidal and tumor inhibitory effects appear to be related to the presence of the —SO—S— linkage and may be the result of —SH inactivation by direct combination or by oxidation of —SH to —S—S—. The decreased uptake of S^{35} L-cysteine by leukemic leukocytes may also be related to —SH inactivation. These effects on malignant cells by an agent which inactivates —SH groups are further suggestive of the importance of —SH metabolism in neoplasia (*10*).

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3 September 1957

Biquartimin Criterion for Rotation to Oblique Simple Structure in Factor Analysis

The last 5 years have seen much effort on the part of workers in the field of factor analysis to develop a completely analytical method for rotating axes to what Thurstone (*1*) called "simple structure." Such a method would supplant the largely subjective, graphical methods which have been in wide use for at least 20 years. In 1953 I published (*2*) a method for the general case of oblique axes, but the results were not satisfactorily close to those achieved by the best graphical methods. At about the same time Saunders (*3*), Neuhaus and Wrigley (*4*), and Ferguson (*5*) independently proposed what Neuhaus and Wrigley called the "quartimax" method, which yielded an approximation to simple structure under the restriction of orthogonality. This method is mathematically equivalent to my method, under the stated restriction. Kaiser (*6*) showed that part of my solution can be achieved by the use of a characteristic equation; he also presented (*7*) a further criterion for the orthogonal case, called the "varimax" criterion since it depends on maximizing the variance of squared factor loadings. Pinska and Saunders (*8*) suggested a variant of their criterion for the oblique case, and Kaiser (*9*) generalized his varimax method for the oblique case.

This report (*10*) presents the "biquartimin" criterion for simple structure in the oblique case. When applied to several "school problems" such as Thurstone's box problem (*1*, p. 229), it yields results which appear to be closer to graphical solutions than those yielded by other analytical approaches. The complete evaluation of this and other methods awaits the development of parallel high-speed computational systems and their application to a wide variety of data.

To introduce the biquartimin criterion, it may help to refer to my original method (*2*) as the quartimin, since it depended upon the minimization of the sum of the cross-products of squared factor loadings and thus involved terms of the fourth degree. Let the $n \times m$ matrix of the initial factor loadings be denoted F, where n is the number of variables and m is the number of factors. Then, the quartimin method finds a transformation matrix Λ such that it will be true of the elements v_{jp} of the resulting matrix $V = F\Lambda$ that

$$\sum_{p<q}^{m} \sum_{j=1}^{n} v^2_{jp} v^2_{jq} = \text{a minimum},$$

where $j = 1,2, \ldots n$, and $p, q = \text{A,B}, \ldots m$. The rationale offered for the quartimin criterion depended on the fact that simple structure requires a maximum number of zero or near-zero entries in V.

Kaiser's (*9*) generalization of his varimax criterion to the oblique case might be called the *covarimin* criterion, since it requires that the sum of the covariances of squared factor loadings be a minimum; that is, that

$$\frac{1}{n} \sum_{p<q}^{m} \sum_{j=1}^{n} v_{jp} v_{jq} = \text{a minimum},$$

where

$$v_{jp} = (v^2_{jp} - \overline{v^2_{jp}})$$

The covarimin criterion is closely related to the quartimin criterion but corrects for the mean value of the squared factor loadings. Thus, the latter utilizes the deviations of squared factor loadings from zero while the former utilizes deviations from their mean value.

Experimentation with the quartimin and covarimin criteria as applied to several sets of real or hypothetical data revealed that the presence of factorially complex variables created almost precisely opposite disturbances, the quartimin axes being too highly correlated and the covarimin axes being too much separated. The centroids of corresponding quartimin and covarimin transformation vectors proved to be very close approximations to the results of graphical solutions, but this type of solution (although otherwise acceptable) was rejected because it would entail more than twice the normal amount of computation and the possibility of difficulty in identifying corresponding vectors.

Table 1. Data for Thurstone's "box problem" (*1*, p. 229): transformation matrix (Λ) obtained by the analytical biquartimin method as compared with that obtained by Thurstone by graphical methods.

	X	Y	Z
	Biquartimin criterion		
I	.450	.434	.473
II	−.862	.237	.597
III	.234	−.869	.648
	Thurstone's solution		
I	.483	.466	.479
II	−.834	.254	.560
III	.267	−.847	.675

The biquartimin criterion combines the advantages of the quartimin and covarimin methods by requiring that the expression

$$\sum_{p<q}^{m} \left[\sum_{j=1}^{n} v^2_{jp} v^2_{jq} + \sum_{j=1}^{n} v_{jp} v_{jq} \right]$$

be a minimum. It doubly satisfies the requirement of parsimony (*5*) in that the sum of cross-products of squared factor loadings must be minimized along with the sum of cross-products of deviations of squared factor loadings from their mean values.

In one of several possible computational schemes, the biquartimin criterion can be expressed as the sum of the off-diagonal elements of a symmetric matrix composed of quadratic forms—that is,

$$\sum_{p<q}^{m} \lambda_p \mathrm{H}_q \lambda'_p = \text{a minimum},$$

where λ_p is a transformation vector of Λ and

$$\mathrm{H}_q = 2n \sum_{j=1}^{n} (\lambda_q \mathrm{F}'_j \mathrm{F}_j \lambda'_q) \mathrm{F}'_j \mathrm{F}_j - (\lambda_q \mathrm{F}' \mathrm{F} \lambda'_q) \mathrm{F}' \mathrm{F},$$

where F_j is the jth row of F. The solution for the minimum value must be made iteratively. For any one stage of the iterations, designate the vector to be solved for as x and any one of the remaining vectors as r, then determine

$$\lambda_x (\Sigma \mathrm{H}_r) \lambda'_x = \text{a minimum}$$

by determining λ_x as the latent vector corresponding to the smallest latent root of the symmetric matrix $(\Sigma \mathrm{H}_r)$. (In

starting the computations, each of the vectors λ_r may be chosen arbitrarily.) In the next stage of the iterations, the λ_x just solved for becomes one of the λ_r's, and another vector becomes the λ_y to be solved for. In the small problems which have been worked thus far, with m no greater than 4, convergence has been reasonably rapid, depending in large measure on the apparent "cleanness" of the factorial structure.

As an illustration of results obtained, Table 1 compares the biquartimin solution with that obtained by Thurstone (*1*, p. 229) by graphical methods for his "box problem." Corresponding transformation vectors from the two solutions are about 3° apart.

The principle utilized by the quartimin criterion could be applied easily to the special case where one requires orthogonality. This has not yet been done; at any rate, it would seem that the criterion of simple structure should alone determine to what extent any given set of data approaches orthogonality. Like other oblique solutions, the biquartimin criterion allows complete freedom in this respect.

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19 July 1957

Chemoprophylaxis with Diazouracil of Poliomyelitis in Mice

One approach to the chemotherapy of virus diseases has been the use of various antimetabolites to interfere with nucleic acid metabolism (*1*). Since the publication of the reports on the action of benzimidazole against poliomyelitis in mice (*2*), investigation of compounds of this nature has been pursued in this laboratory (*3*) as a possible method for chemoprophylaxis. One of the substances tested was diazouracil, which had been found to have some activity against certain viruses (*4*). This report presents evidence about the effectiveness of diazouracil in the prevention of paralytic poliomyelitis in mice.

Table 1. Chemoprophylaxis of poliomyelitis with diazouracil in mice. Mice were inoculated intraperitoneally with an estimated 10 ID_{50} of MEF_1 poliomyelitis virus. Diazouracil treatments (10 mg/kg day, intraperitoneally) were given for 4 days, beginning one day before virus inoculation.

Treated No. surviving/ No. inoculated	Control No. surviving/ No. inoculated	Survival index
	Sample No. 1	
11/19	3/20	5.0
6/20	2/20	2.0
8/20	2/19	1.7
7/19	1/20	1.6
	Sample No. 2	
6/20	0/20	1.7
8/19	5/20	1.9
	Total	
46/117 (39%)	13/119 (11%)	

In these studies, mice of the Webster strain weighing less than 12 g were inoculated intraperitoneally with 0.2 ml of a 10 percent suckling-mouse brain suspension of the MEF_1 strain of type II poliomyelitis virus, approximately 10 ID_{50}. Mice were treated intraperitoneally with diazouracil (*5*) at the rate of 10 mg/kg day for 4 days beginning the day before virus inoculation; however, on the day of virus administration, treatment was given subcutaneously. Control animals were treated similarly with equal volumes of buffered saline. Mice were examined daily for paralysis throughout an observation period of 21 days.

Data from several experiments with diazouracil are presented in Table 1. The results are expressed as the ratio of the number of animals surviving on the 21st day to the number of animals inoculated. A survival index was calculated from the ratio of the harmonic mean of the survival time of the treated group to that of the control group, with a favorable response in terms of prevention or delay indicated by ratios greater than 1 (*6*). In all experiments, diazouracil reduced the incidence or delayed the onset of paralysis in mice inoculated with poliomyelitis virus. Thus, in the first experiment, treatment with diazouracil reduced the incidence of poliomyelitis from 85 percent (three survivors of 20 mice inoculated) to 42 percent (11 of 19 surviving), with harmonic mean survival times of 4.0 and 20.2 days, respectively. When the results of these tests were combined, it was found that only 13 of 119 control animals survived, compared with 46 of 117 treated animals—a difference significant at the 1 percent level (*7*). When treatment with diazouracil was begun on the day of virus inoculation or thereafter, it was less effective. No protection was observed when intraperitoneal treatment with diazouracil was started the second day after virus infection or when diazouracil was given orally at the rate of 100 mg/kg day for 4 days beginning the day before virus inoculation.

In contrast to its action in mice, diazouracil did not protect monkeys. When infected orally with the Mahoney strain of poliomyelitis virus, 6 of 6 monkeys in each of two control groups developed paralysis, as did a group which was treated intraperitoneally with four daily doses of 5 mg of diazouracil per kilogram each, beginning the day before virus inoculation, while in a group treated intravenously with five daily doses of 2.5 mg/kg, the morbidity was 5 of 6.

Although the effectiveness of diazouracil is compatible with the assumption that analogs can be used to interfere with the nucleic acid metabolism involved in virus replication, it remains to be demonstrated that this is the mechanism of the chemoprophylactic action of the compound against poliomyelitis in mice.

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25 July 1957

New Low Chromosome Number for Plants

Previously, the lowest chromosome number reported for plants was $X = 3$. This number occurs in *Crepis*, *Crocus*, and *Ornithogalum* (*1*). In the process of a biosystematic study of the *Blepharodon* section of *Haplopappus* several species have been found to have low chromosome numbers. Of particular interest, however, is *Haplopappus gracilis* (Nutt.) Gray. This is a small annual composite that ranges from southern

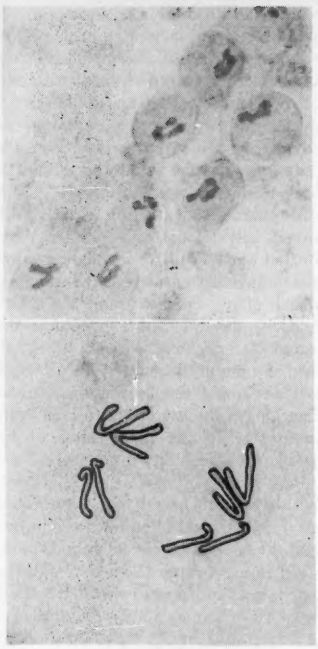

Fig. 1 (Top). Early first anaphase of meiosis showing two pairs of chromosomes. Fig. 2 (Bottom). Late mitotic anaphase showing chromosome number and morphology. The chromosomes have been outlined with India ink.

Colorado and Utah to the state of Durango in Mexico, and west to the eastern border of California (*2*). Seeds of this species were germinated in the greenhouse in March, and the plants flowered in July.

For cytological study, immature heads were fixed for 48 hours in a mixture of two parts of absolute ethyl alcohol to one part of propionic acid. Because of the small size of the disc flowers, the anthers were not dissected out. Instead, entire corollas were macerated and then squashed in iron propionocarmine stain. Usable slides were made permanent by withdrawing the stain from one side of the cover glass with filter paper while a Venetian turpentine and alcohol mixture was introduced from the other side.

In a study of meiosis of two plants of *H. gracilis,* only two pairs of chromosomes were observed at the beginning of the first anaphase (Fig. 1). Other stages of meiosis were studied and found to be normal. The plants produced approximately 100 percent normal pollen.

Several stages of mitosis were observed in the floral tissue. In a mitotic anaphase (Fig. 2) four chromosomes can be seen near each pole. These are morphologically distinct. One pair of homologs has the centromere located in a near-median position with only slightly unequal arms, while the other set has a subterminal centromere with correspondingly unequal arms. For reference in future studies of *H. gracilis,* the chromosomes with the near-median centromere are designated as *A* and those with the subterminal centromere as *B*. The length of chromosome *A* at mitotic anaphase in 12.5 μ, and that of *B* is 7.5 μ.

Apparently the basic chromosome number for the *Blepharodon* section of *Haplopappus* is $X=2$, as found in *H. gracilis.* This is the lowest chromosome number known in plants at the present time. In related species of *Haplopappus* I have found $n=4$ in *H. spinulosus* ssp. *typicus* Hall (*3*), $n=4$ and 6 in *H. spinulosus* ssp. *cotula* (Small) Hall, and $n=8$ in *H. nuttallii* Torr. and Gray (*4*). It is interesting to note that Hall (*2*) considered *H. gracilis* to be morphologically the most advanced annual member of its section. It thus appears that evolution of this species has occurred on the diploid level.

In addition to being an object of curiosity, *H. gracilis* may prove to be an excellent cytogenetic tool. Seeds of the species germinate easily, and the plants grow well under greenhouse conditions. Preliminary tests in hybridization indicate that the species is self-sterile. The preceding characteristics plus the fact that *H. gracilis* has only two relatively large pairs of chromosomes at meiosis, and thus two linkage groups, should make it a very suitable plant for experimental study.

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20 August 1957

Biological Synthesis of Cellobiose

A cellobiose phosphorylase from *Clostridium thermocellum* acts upon cellobiose to produce equimolecular quantities of glucose and glucose-1-phosphate (*1*). The enzyme can be liberated from bacterial cells that have been grown on either cellulose or cellobiose by grinding them with alumina or glass beads. The reaction catalyzed by this enzyme is reversible and, under conditions similar to those used for the biological synthesis of sucrose (*2*) and maltose (*3*), will yield cellobiose from a mixture of glucose and α-D-glucose-1-phosphate.

In a representative experiment, a mixture containing phosphorylase and the substrates for cellobiose synthesis was prepared. It contained the following: 0.45 ml of maleate buffer at *p*H 7.6; 0.70 ml of enzyme, 17 mg of protein per milliliter; 0.15 ml of 0.5*M* sodium fluoride; 0.15 ml of 0.1*M* magnesium sulfate; 0.15 ml of 0.14*M* sodium thioglycollate; 0.45 ml of 0.1*M* glucose; and 0.45 ml of 0.1*M* glucose-1-phosphate. One milliliter of this mixture was placed in each of two tubes. One tube was immediately immersed in boiling water for a period of 3 minutes to inactivate the enzyme. The second tube was incubated at 37°C for 6 hours, and then the enzyme was inactivated by heating the tube in boiling water. The mixtures were freed of salts by shaking the tubes with small amounts of ion-exchange resins (Chempro C-20 and Duolite A-40), and then chromatographic examinations for sugars were made. The fluids removed from the resins were evaporated to dryness at room temperature in a vacuum desiccator. The sugars in the residue were taken up in 1 ml of 95-percent ethanol at room temperature. The sugars in the alcoholic solution were separated and tentatively identified by paper chromatography by use of S and S 589 green ribbon paper and a descending butanol-pyridine-water solvent (*4*). A control solution containing 0.5 μmole each of glucose and cellobiose was placed on the same paper. The solvent was permitted to drip from the edge of the paper to obtain good separation of glucose and cellobiose. The paper was dried, and the sugar spots were developed by treating the paper with an isopropyl alcohol solution of phthalic acid and aniline (*5*) followed by heating for 10 minutes at 100°C.

Glucose and cellobiose separated well; glucose but no cellobiose was present in the zero-time tube; and both glucose and cellobiose were present after 6 hours' incubation of glucose and glucose-1-phosphate in the presence of cellobiose phosphorylase.

The synthesis of cellobiose was further demonstrated in a similar experiment in which larger quantities of glucose, glucose-1-phosphate, and enzyme were used. Proteins were removed from the reaction mixture by treatment with trichloroacetic acid. The mixture was then made alkaline, and the glucose phosphates were precipitated by the addition of four volumes of cold ethanol. The alcohol was evaporated, and the partially purified sugars were taken up in water. A sufficient quantity (about 10 mg) of cellobiose was obtained to prepare an osazone (*6*). The cellobiosazone was separated

by dissolving the sample in hot water and recrystallizing from pyridine. Its melting point was approximately 195°C. It appeared to be identical with known cellobiosazone that was examined at the same time.

This formation of a β-glucoside from an α-glucose derivative confirms the observation of Fitting and Doudoroff (*3*) that the synthesis of a disaccharide by this type of a phosphorylase involves a Walden inversion of the glucose-1-phosphate (*7*).

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12 August 1957

Mechanism of Agglutination of Macaca rhesus Erythrocytes by Human Hepatitis Serum

In an attempt to find a useful laboratory method for the assay of the human hepatitis virus, we have investigated the recently described (*1*) phenomenon of *Macaca rhesus* erythrocyte agglutination. Although this reaction was considered to be the result of a nonspecific antibody combination, we examined the possibility that it might be a direct effect of a virus.

Hepatitis sera were obtained from local hospitals and from the Communicable Disease Center, U.S. Public Health Service, Chamblee, Ga. Many of the diagnoses were proved by autopsy or biopsy, and all had extensive laboratory support. For comparison, serum was obtained from other medical cases, both with and without jaundice, and from normal blood-bank donors of the various major blood groups and Rh types.

Blood from a small group of normal rhesus monkeys was drawn into 4 percent sodium citrate, and the cells were washed with large volumes of 0.9 percent normal saline. Hemagglutination reactions were carried out in twofold serum dilutions in saline; 0.2-ml volumes, measured with calibrated pipettes, were used. Equal volumes (0.2 ml) of the washed 1 percent rhesus cells were added, and the shaken tubes were refrigerated at 4°C for 2 hours. Agglutination was determined by the settling patterns and from the cohesion of the cells when the tubes were gently tilted.

The distribution of hemagglutination titers (reciprocal of the highest dilutions showing clear agglutination) among controls and medical patients, including those with clinical jaundice of nonviral origin, was homogeneous and ranged from less than 4 to 64. Neither blood type nor jaundice caused any detectable variation from the normal range. Saline-typing sera of the major groups and Rh types (anti-C, anti-D and anti-E) failed to produce any agglutination of the monkey cells.

Among the sera from hepatitis cases, both infectious hepatitis (IH) and serum hepatitis (SH) sera gave titers ranging into the thousands, usually 1024 or 2048, and occasionally higher. When serial samples were taken, the SH sera retained the higher titers both initially and throughout the recovery period. Some continued to show high readings even when no further clinical or biochemical evidence of hepatitis could be detected. On the other hand, the titer of the IH cases dropped off at approximately the same rate as the other criteria of the disease.

Of the 51 sera from clinically active cases of viral hepatitis tested, all showed titers exceeding 128. More than half of these were 1024 or over. In 22 sera from convalescents showing no clinical symptoms or biochemical abnormalities, about half were in the normal range, while the others ranged between 128 and 2048. There appeared to be no correlation between any of the clinical or biochemical tests and the level of hemagglutination.

Among convalescent SH cases, two with no detectable agglutination titers were mixed (in equal volumes) with both active IH and SH material and incubated at 37°C. The agglutination titers of all SH sera tested were rapidly and uniformly reduced (Fig. 1); but no decrease of effect was seen with any IH serum. However, none of the sera from convalescent IH cases tested had any effect on either IH or SH, under the above conditions. Commercial gamma globulin, which has a reproducible titer of 32, also failed to reverse the effect of either IH or SH material. Of ten normal sera tested, none had ability to reduce the hemagglutinating activity of any serum studied.

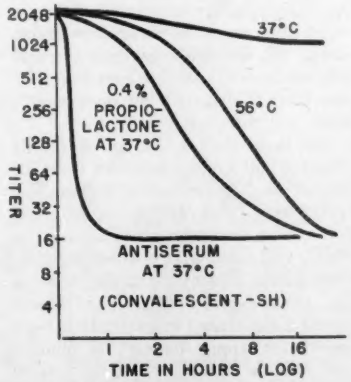

Fig. 1. Effect of various treatments on the agglutination of *Macaca rhesus* erythrocytes by the serum of an acute case of serum hepatitis (SH).

High-titer IH and SH sera were incubated at 37°C and at 56°C for up to 30 hours. Periodically, samples were tested for hemagglutination. Typical results are seen in Fig. 1. The lower temperature had only a slight effect, while the higher temperature effectively reduced the titer. The lower the initial titer, the more rapid was the reduction by heat.

A number of active serum samples were incubated under various conditions with β-propiolactone, a compound known to be strongly viricidal (*2*). The rate of disappearance of the hemagglutination titer depended on the concentration of the chemical and the temperature of incubation. A typical result is shown in Fig. 1. Higher concentrations at elevated temperatures were more rapidly effective. Refrigeration of serum containing 0.4 percent β-propiolactone caused only a slight loss of activity during the test period.

All the mixtures that were studied contained penicillin and streptomycin, and all were tested for sterility at the end of the experiment. No contaminations were observed, nor did the antibiotics alone interfere with the hemagglutination reaction.

In some cases treatment with heat, β-propiolactone, or serum from convalescents reduced the hemagglutination titers of active sera to less than four (the minimum test level). Usually, however, as shown in Fig. 1, there was an irreducible minimum below which the titer would not fall. In control runs, in which the three test treatments were compared in nonhepatitis sera that showed appreciable titers (32 to 64), no diminution of hemagglutination could be achieved. These methods also failed to lower the agglutinating ability of antisera experimentally prepared against the same monkey erythrocytes.

Our data could be explained by the presence of two independent mechanisms for the agglutination of rhesus erythrocytes by human sera. Clear low levels of effect are found in most human serum, regardless of the source and these seem to be of a nonspecific nature. But superimposed upon these levels there exists a

greater effect that may be reversed by at least three mechanisms known to be capable of reducing virus activity. These same mechanisms are without effect when tested against nonhepatitis serum, normal gamma globulin, or antiserum prepared with the test erythrocytes.

The sera from convalescents which were effective in reducing the effect of the hepatitis sera showed clear specificity, although we have not yet found a serum capable of reversing the IH serum effect.

It is also clear that at least the higher agglutination titers could not be accounted for by any of the major Rh blood types. This agrees with an early observation of Wiener (*3*) that human Rh negative sera capable of reacting with Rh human erythrocytes were ineffective against rhesus red cells.

It is possible, therefore, to account for the high hemagglutination titers as a virus effect and to remain consistent with the few known characteristics of the hepatitis viruses (*4*).

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12 August 1957

Sulfa Compounds in Reversible Inhibition of Sperm Metabolism by Carbon Dioxide

A recent report from our laboratory has presented evidence of reversible inhibition at 37°C of motility and glycolysis of bovine spermatozoa by anaerobiosis and relatively high tensions of CO_2 (*1*). The potential for maintenance of fertility of spermatozoa inhibited by CO_2 during storage for 1 week under the rigors of normal room temperature has been demonstrated (*2*).

The Illini variable-temperature diluent (IVT) used in the studies of fertility contained 20.0 g of sodium citrate dihydrate, 2.1 g of sodium bicarbonate, 0.4 g of potassium chloride, 3.0 g of sulfanilamide, 1 million units of penicillin G, and 1.0 g of dihydrostreptomycin sulfate per liter. The sulfanilamide, penicillin, and streptomycin were first added to this diluent primarily as antibacterial agents to prevent the rapid bacterial growth that otherwise occurs during storage of bull semen at room temperature (*3*). In addition to its bacteriostatic effect in diluted semen, sulfanilamide has been known for more than 10 years to inhibit respiration of spermatozoa at 37°C and to inhibit aerobic utilization of carbohydrate by bull semen that is stored in yolk-citrate at 5°C (*4*). Sulfanilamide improved the fertility of semen used routinely for artificial insemination of cattle (*5*). Thus, sulfanilamide has been an ingredient of most diluents used for storage of semen for several years. However, only recently has it been found that sulfanilamide also has a supplementary effect on the inhibition of anaerobic glycolysis brought about by relatively high levels of CO_2 in N_2. Neither penicillin nor streptomycin exerts such a marked inhibitory effect.

Table 1. Carbon dioxide evolved per 10^8 spermatozoa from bicarbonate during 4 hours at 37°C in Illini variable temperature diluent (IVT), supplemented with 0.02*M* sulfanilamide or 0.001*M* Diamox in N_2 plus CO_2 and CO_2 only.

Carbon dioxide (%)	IVT only (μl)	IVT and sulfa (μl)	IVT and Diamox (μl)
5	97	50	49
50	30	13	13
100	14	8	6

The function of sulfanilamide as a competitive inhibitor of *p*-aminobenzoic acid is recognized (*6*), as is its inhibition of phosphatases (*7*) and of carbonic anhydrase of animal origin (*7*). Zinc (*8*), a known component of carbonic anhydrase, and phosphatases have been found in bull semen.

It is the purpose of this report to present the evidence that sulfanilamide supplements the inhibition of metabolism by CO_2. Another compound, Diamox, or 2-acetylamino-1,3,4-thiadiazole-sulfonamide (*9*), which is considered as a specific inhibitor of carbonic anhydrase (*7*), also inhibits glycolysis but does so at a lower concentration.

For these studies, 0.2 ml of freshly collected bull semen, containing from 200 million to approximately 450 million sperm cells, was added, in Warburg flasks, after temperature equilibration to 37°C, to 1.0 ml of the Illini variable-temperature diluent containing none of the antibacterial agents. The diluent served as the control when the flasks were gassed for approximately 10 minutes with CO_2 or with N_2 containing 5 or 50 percent CO_2. The diluent in other flasks contained sulfanilamide (0.02*M* or Diamox (0.001*M*). The results reported in Table 1 are the mean cumulative evolution of CO_2 from bicarbonate by 10^8 spermatozoa for three semen samples during a 4-hour incubation at 37°C.

The presence of sulfanilamide and Diamox depressed the glycolysis of the spermatozoa to a level much below that in the Illini variable-temperature diluent alone at all levels of CO_2. Most of the glycolytic activity in the presence of these two additives at the 100-percent-CO_2 level occurred during the first 15 minutes of incubation. When either sulfanilamide or Diamox was used, the inhibition of glycolysis was as effective in 50 percent N_2 and 50 percent CO_2 as that occurring in the diluent under an atmosphere of pure CO_2. The recovery of spermatozoan motility after the incubation and after aeration upon opening the flasks was optimum in the diluent alone and in that with sulfanilamide added but was depressed slightly by Diamox, the mean values being 55, 55, and 44 percent, respectively. Diamox levels higher and lower than the 0.001*M* level were not as effective in controlling glycolysis and did not improve the recovery of spermatozoan motility upon aeration after incubation.

The effect of sulfanilamide on the glycolytic activity of CO_2-inhibited spermatozoa has been repeatedly confirmed. In comparisons with 15 additional semen samples, the number of microliters of CO_2 produced per 10^8 sperm cells in 4 hours at 37°C in the absence and presence of sulfanilamide were 95 and 56, 24 and 12, and 11 and 8 under 5, 50, and 100 percent CO_2, respectively.

These supplementary inhibitory effects of sulfanilamide and Diamox were not due to differences in *p*H. The mean final *p*H's of the flask contents were 6.7, 7.0, and 6.7 in 5-percent CO_2 for the Illini variable-temperature diluent alone, with sulfa added, and with Diamox added, respectively. With 50-percent CO_2 the final values were 6.8, 6.6, and 6.7; with 100-percent CO_2, they were 6.7, 6.7, and 6.5, respectively.

The above results confirm the earlier report of CO_2 inhibition of glycolytic activity of spermatozoa and show that at least two sulfa compounds increase the inhibitory effect of CO_2. The mechanism of sulfa inhibition as well as CO_2 inhibition of spermatozoan glycolysis remains to be identified (*10*).

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12 August 1957

New "Fast" Hemoglobin Associated with Thalassemia

To date, through electrophoresis, four variants of human hemoglobin with higher anodic mobility than normal adult hemoglobin have been recognized (hemoglobins H, I, J, and K) (*1*). Recently we identified another "fast" hemoglobin in the cord blood of a full-term infant. Through paper electrophoresis of hemoglobin solutions in both barbitone (*p*H 8.8; Γ/2, 0.025) and phosphate (*p*H 6.5, 0.03*M*) buffer, two spots were obtained: a large one, corresponding to a mixture of hemoglobins F and A, and a smaller one which migrated toward the anode. Comparison of the latter with hemoglobin H (*2*) proved that H is of higher anodic mobility in both alkaline and acid buffers (Fig. 1); it was possible to separate the artificial mixtures in both buffers. The new hemoglobin also differs from hemoglobin I (*3*); at *p*H 6.5, hemoglobin I showed almost no separation from hemoglobin A, while the new hemoglobin migrated clearly away from A. Hemoglobin I, of all fast hemoglobins with the exception of H, takes a more anodic position in acid buffer (*4*). Consequently, the new hemoglobin differs from both hemoglobins J and K, which on paper at *p*H 6.5 resolve less than hemoglobin I (Fig. 2).

The new hemoglobin is not alkali-resistant. At the birth of the infant it amounted to 14 percent of the total (determined by elution), the content of hemoglobin F being 60 percent, and of A, 26 percent. The infant was neither anemic nor icteric. During the next 3 months there was a progressive reduction in the amount of the fast fraction present to 4 percent, and of hemoglobin F, to 20 percent.

An investigation of the infant's family showed that the mother has thalassemia major, while the father has thalassemia minor. Three of the grandparents, all having thalassemia minor, originated from the same village in Asia Minor (Fig. 3). Consanguinity is denied by them. The results of genotyping with eight antisera were consistent with the claimed parentage. Fast hemoglobin was found in neither the parents nor in the relatives who were examined. The hematologic and genetic data indicate that the infant definitely carries one, and possibly two, doses of the thalassemia gene (*5*).

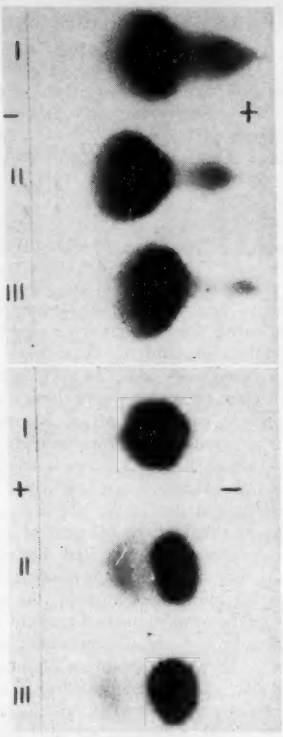

Fig. 1. Electrophoresis of (I) hemoglobin I + A, (II) new hemoglobin and A + F, (III) hemoglobin H + A. (Top) Electrophoresis in barbitone buffer (*p*H 8.8; Γ/2, 0.025; 10 hr, 0.3 ma/cm; Whatman No. 3). (Bottom) Electrophoresis in phosphate buffer (*p*H 6.5; 0.03 *M*; 5 hr, 1 ma/cm, Whatman No. 3).

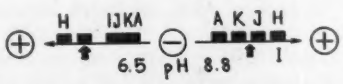

Fig. 2. Schematic representation of relative positions of "fast" hemoglobins as found through paper electrophoresis. Arrows indicate the position of the new fraction.

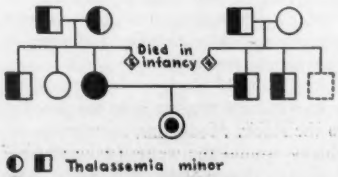

Fig. 3. Family tree showing occurrence of thalassemia and new "fast" hemoglobin.

The presence of the abnormal hemoglobins S, C, D, E, I, J, and K is genetically determined, and the abnormal component is always found in at least one parent of affected persons. Hemoglobin H differs from these in that it appears in the phenotype only in association with the thalassemia gene (*2*, *6*). The appearance of the hemoglobin under study could be compared with the genetic behavior of H; this hemoglobin differs from H, however, since neither the father, who has one dose of the thalassemia gene, nor the mother, who has a double dose, shows the abnormal component. The data on hand are suggestive that this may be an abnormal form of fetal hemoglobin, hitherto not recognized, which found expression because of its association with the thalassemia gene. An alternative explanation could be that we are in the presence of a mutation.

The thalassemia gene, although it is not responsible for the synthesis of a specific abnormal hemoglobin, is considered to be the causative factor of such alterations of the hemoglobin pattern as (i) the persistence of a high percentage of fetal hemoglobin beyond infancy; (ii) the increase of hemoglobin A_2 several times above normal (*7*, *8*); (iii) the increased production of hemoglobins S, C, E, and possibly G (*9*) when associated with the respective genes thereof; and (iv) the "phanerosis" of hemoglobin H. In the present case, a further alteration seems to have been caused by the thalassemia gene (or genes), the abnormal component being already present at birth. Further investigations on new-born infants likely to be affected by thalassemia are necessary (*10*).

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5 September 1957

Book Reviews

Man Among the Stars. Wolfgang D. Müller. Translated from *Du wirst die Erde sehn als Stern.* Criterion, New York, 1957. 307 pp. Plates. $4.95.

This is an unusual book. To my knowledge, it is the first book to be almost wholly dedicated to a justification of man's plans for the conquest of space. In such a book some historical background is necessary, along with a survey of our present stages in research and development toward such a goal. This is given in concise descriptions of rocketry, the effects of war on development, the present satellite program, and future plans for space stations and space ships. The author implies that the present position of the intercontinental ballistic missile programs may hinder our development of space travel and believes that a straightforward program would be better.

The spread of man over the earth, the exploration of faraway lands, the opening up of the Americas and of our Far West are attributed to built-in drives common to most members of the human race. On this basis, the conquest of space is inevitable, barring a major catastrophe to mankind. Even hysteria, such as the reaction to Orson Welles' broadcast of a play based on H. G. Wells' *War of the Worlds,* in October 1938, and the present controversy over unidentified foreign objects, appears to be part of the same pattern of reaction to the exploration drive. Similar and analogous waves of hysteria appeared during the great Age of Exploration, several hundred years ago.

Careful discussions of the effects of the discovery of the telescope on men's minds and speculations regarding gravity, the possible existence of extraterrestrial beings, and the role of religion in the event that such beings should be found to exist are intelligently evaluated on our present level of development. The Roman Catholic groups are far ahead of the other religious organizations in anticipating extraterrestrial life forms and have speculated on their relationship to man and man's personal religious beliefs.

Müller sees the conquest of space as an opportunity for a new dedication of mankind similar to the dedication that arose during the religious drives, the Age of Exploration, and the Crusades.

"We need a new idea," he says, "which might open up such an opportunity, one that would appeal to all of humanity and that could transform our view of the world, an idea with such a good chance of realization that it will fire the human imagination" (page 299).

The spiritual effect on man of such an enterprise, the unlimited horizons, and, if intelligent beings are found, the sense that man is not alone in the universe might serve to unravel some of the conflicts in our age of confusion. It is proposed that such a venture might act prophylactically on man, enabling him to achieve a saner viewpoint on life, and that it could constitute an alternative to the "shock-therapy" or "racial-lobotomy" impact that a nuclear war would have on the survivors, if any, in achieving racial sanity. Müller's implications bear consideration and may have merit.

THOMAS S. GARDNER
Hoffmann-La Roche, Inc.

Reading the Landscape. An Adventure in Ecology. May Theilgaard Watts. Macmillan, New York, 1957. x + 230 pp. Illus. $4.75.

An author, having selected this excellent and challenging title, might have followed either of two courses. The first would have been a statement of principles, shrewdly selected from geology, soil science, and ecology, that the traveler could see exemplified in his journeys. The other, more concrete, is here adopted. It amounts to making the reader a companion on field trips, pointing out specific situations, and showing how to interpret them.

While the 13 chapters deal with landscapes in areas ranging from the Smokies to the Rocky Mountains, interest centers chiefly around the western lake states—Indiana, Illinois, and Wisconsin. Here May Theilgaard Watts, naturalist at the Morton Arboretum in Lisle, Illinois, is completely at home, having had the advantage of training under the late Henry Cowles at Chicago.

It is natural, then, that she should emphasize the ecological communities which clothe the landscape, although the geological and climatic influences that shape it are by no means neglected. The net result is an attractive and usable volume, informally written and illustrated with pen sketches.

I was especially intrigued by two chapters, near the end of the book, which show how ecological analysis can be divertingly applied to simple situations. One of these is the reconstruction of the history of an abandoned schoolhouse; the other, of the effect of changing fashions on landscape design about an old homestead.

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The Wonder of Snow. Corydon Bell. Hill and Wang, New York, 1957. xvi + 269 pp. Illus. $5.

The multifaceted crystals of winter's wonderland, the microscopic and macroscopic magic that forms a flurry of flakes or a "landscapeful" of snow to delight the eye of an artist, provide Corydon Bell with his subject matter. He roams the globe, from pole to pole, and the realms of poetry and prose, wherever snow is featured. He travels the blooming deserts, fruitful because of snow-fed rivers and canals, climbs to mountain crags where avalanches are massive parcels of concentrated energy, flies into and above the clouds where nature works her physical and chemical snow-wizardry, and visits scientific laboratories where meteorologists strive to understand and duplicate earth's atmospheric snow machine.

For all its wide coverage, the book reads well and provides the inquisitive mind with much food for thought. There is evidence that snow has been falling somewhere on earth for much, much longer than men have inhabited this planet. In fact, man probably owes much of his vigor and aggressiveness in organizing the world to serve his needs to the nipping cold and biting wind that accompany winter pageantry. Yet the study of snow in a scientific manner is of comparatively recent origin.

Meteorology as a science, with a background of sufficient data to improve on the old farmer's prognostications, is only some 75 years old at most. (And when the weatherman is wrong, some folks say the science "hasn't been born yet.") Only since 1954 has Ukichiro Nakaya's monumental work *Snow Crystals* been available, with its classification of the crystals into seven basic types and information about how these types originate.

Corydon Bell has selected a well-bal-

anced collection of photographs and drawings to illustrate this book, including some from the work of farmer-photographer Wilson Bentley of Vermont, whose photomicrographs of snow and frost were collected over a period of 40 years before Nakaya. Through his acknowledgments and a good index, it is apparent that he has drawn upon a wide knowledge and familiarity with snow-lore and snow-science. He makes room for the Abominable Snowman and Alfred Wegener, Homer and Chaucer, Olaus Magnus and Vincent Schaefer, Byrd and Hobbs, "Snowshoe" Thompson and Langmuir.

This is by no means a textbook or even a technical book, though it treats of a subject that has a technical side. From the very first chapter one knows that the author loves winter and the snow, and before bumping up against the glossary, the average reader will also have an appreciation of what scientists have accomplished in finding out how, when, where, and why it snows, and of the men (other than Bell) who have left "footprints in the snows of time" for those who would follow. *The Wonder of Snow* belongs on every high-school science reference shelf and will make a splendid gift for youngsters and oldsters alike who have a fondness for nature and out-of-doors—and snowflakes.

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H. A. Lorentz, Impressions of His Life and Work. G. L. de Haas-Lorentz, Ed. North-Holland, Amsterdam, 1957. 172 pp. + plates. $3.

The purpose of this volume is to give an impression of one of the greatest physicists of the first quarter of our century.

Most of the book is taken up by the very personal reminiscences of Lorentz's eldest daughter, herself a physicist and the wife of a physicist. Interspersed between these reminiscences are contributions by friends and pupils. Fokker gives a semipopular account of Lorentz's *oeuvre*, Van der Pol assesses the importance of Lorentz's work in the field of modern telecommunication, Thysse tells the fascinating story of how Lorentz calculated the influence of the proposed reclamation of most of the Zuyder Zee on the behavior of the tides in the remainder, and Casimir discusses the influence of Lorentz's ideas on modern physics.

One is left with a very definite picture of the man and physicist Lorentz, not least through the short contributions by Einstein (especially written for this volume) and Ehrenfest (a translation of his speech at Lorentz's funeral). Anybody interested in the history of science and in scientists as human beings will read this volume with great profit.

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Electricity and Magnetism. B. I. Bleaney and B. Bleaney. Clarendon Press, Oxford, England, 1957 (order from Oxford University Press, New York). xiv + 676 pp. Illus. $10.10.

Here is just the book for a scientist who cultivates another field to have on his shelf for easy reference. The authors, B. I. and B. Bleaney, both lecturers in physics, at different colleges of Oxford University, hope to fill "the need for an up-to-date text on *Electricity and Magnetism* which would cover the whole field, both the theory and the practice," for their undergraduate students, and a few "chapters have been included which may form part of a graduate course."

They use the word *comprehensive*, which is surely no overstatement, for after eight chapters on "fundamentals at an elementary level," they romp through chapters on alternating-current theory; electromagnetic waves (including filters, transmission lines, and waveguides); electromagnetic machinery; thermionic vacuum tubes (three chapters, and it must be these they had in mind when they spoke of "practice"); and alternating-current measurements. After these come the chapters that I suppose are suitable for a graduate course: theory of the dielectric constant; theory of conduction in the solid state; the atomic theories of paramagnetism, ferromagnetism, and antiferromagnetism; and magnetic resonance. There is a final chapter on units, and I am happy to see that the authors use the rationalized metre-kilogram-second system throughout the book.

It is hardly necessary to remark that a book with this coverage, even a book of nearly 700 pages, can never be profound. But it is surprising how thorough the book can be and still remain readable and easy to follow. This argues careful planning and elimination of nonessentials. I find that when I read in unfamiliar fields, the book is interesting and informative; when I read in fields that I know well, it is clear and accurate.

Definitely this is a book to be taken down off the shelf when some information is wanted on, say, contact potentials or nuclear magnetic resonance. You will find a brief and illuminating section on either. This may be enough, but it may very well be that you will then want to read further in more detailed treatises. This is where Bleaney and Bleaney fail us, for they have missed the opportunity to give lists of references for the inquiring reader. Perhaps in some future edition . . .

You should not approach this book without a previous knowledge of general physics, such as most American colleges give to freshmen. In mathematics, the language of calculus is supposed to be familiar. Vector analysis is used, with a notation nearly enough like the common style to keep one from feeling much annoyance on this score; the appendix is adequate for purposes of review rather than of learning.

The exposition is clear and straightforward. The style is simple, but it has an elegance that we have come to expect of the English universities. *Electricity and Magnetism* is pleasant reading and, in brief, is a book I shall be glad to have for my own frequent use.

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Modern Mathematics for the Engineer. Edwin F. Beckenbach, Ed. McGraw-Hill, New York, 1956. xx + 514 pp. Illus. $7.50.

The Department of Engineering at the University of California has organized a series of lecture courses in modern physics, mathematics, and chemistry. The objective was to acquaint engineers with some late scientific discoveries and to stimulate their application in engineering.

The first set of these lectures—those on physics—was published a few years ago and contained an authoritative, broad, and largely nontechnical presentation of a large part of modern physics, with very little use of the mathematical formalism. The present volume, covering the lecture course on mathematics, has a quite different character. It covers topics which can be treated by differential and integral equations, probability and game theory, and computational methods—topics which form a smaller, though fundamental, part of modern mathematics.

The book contains an "Introduction" (Weller) and is divided into three parts. The first part is called "Mathematical Models." There are chapters on oscillations (Lefschetz), stability theory (Bellman), calculus of variations (Hestenes), and hyperbolic (Courant) and elliptic (Schiffer) partial differential equations. Two chapters are on applications: exterior ballistics (Green) and elastostatics (Sokolnikoff). Obviously these applications were selected because the lecturers happened to be specialists in these topics.

Other, equally interesting, applications to electromagnetism, and so on are not treated.

The second part, entitled "Probabilistic Problems," contains chapters on prediction (Wiener), game theory (Bohnenblust), operations research (King), dynamic programming (Bellman), and Monte Carlo methods (Brown).

The third part, entitled "Computational Considerations," treats matrices, with applications to engineering problems (Pipes); functional transformations (Barnes); conformal mapping (Beckenbach); nonlinear (Morrey), relaxation (Forsythe), and steep descent methods (Tompkins); and, finally, high-speed computing devices (Lehmer).

Most of the authors are well-known masters of their subjects, and they give excellent presentations, which, though condensed, are intelligible and stimulating. It is not to be expected that the chapters will form a homogeneous unit. The requirements for intelligent reading vary from elementary advanced calculus to Lebesgue integration. The chapters can be read independently and contain references for further reading. One of the authors states, disarmingly, that he is a pure mathematician with very little contact with engineering problems. Most of the authors, however, have had extensive experience in applied mathematics and specific engineering applications. On the whole, the volume is warmly recommended to the modern engineer who has a good mathematical background.

EUGENE GUTH
Oak Ridge National Laboratory

Astronomical Optics and Related Subjects. Proceedings of a Symposium. Zdenek Kopal, Ed. North-Holland, Amsterdam; Interscience, New York, 1956. 428 pp. Illus. $12.50.

It is unfortunate that this excellent volume has a rather misleading title. To be sure, all the subjects treated apply to the problems of modern astronomy, but, more than that, most of them apply more broadly, to optics in general. As a matter of fact, many of the contributors to the volume would not designate themselves astronomers.

This is the proceedings of a four-day symposium held at the University of Manchester, England, in April 1955. The aim of the symposium was twofold: to provide a forum for the discussion of certain fields in optics of timely interest and astronomical significance and to strengthen further the liaison between astronomy and optics by bringing current astronomical desiderata to the attention of contemporary optical experts. The symposium was attended by 105 persons from five European countries; America was not represented. In all, 61 papers were presented, and 46 of them are published here, with an excellent 12-page introduction by the editor, Zdenek Kopal, who is professor of astronomy at Manchester, and with brief concluding remarks by J. Rösch of the Observatoire du Pic du Midi. Most of the text is in English, but ten of the papers are in French and two are in German; each of the 12 papers is preceded by a short abstract in English. There is an author index, but no subject index, alas. There are numerous line-drawing illustrations and several excellent plates; the typography is excellent.

The book is divided into seven main sections corresponding to the several sessions of the symposium. Each section contains from four to ten short papers. The subject matter of the first three sections is definitely in the "general physical optics" category: information theory and optics; optical images and diffraction; interferometry and coherence problems. The last four sections are devoted to topics of more special application to astronomy: electronic devices in astronomical optics (including both photoelectric photometry and the new and promising television techniques) that are supplanting photography in many applications; resolution problems and scintillation or "seeing" as the deleterious, irregular refraction by the earth's atmosphere is commonly called; wide-angle optical systems and aspheric surfaces, of such practical importance in modern astronomical telescopes; and filter photography, in which both dye filters and interference (thin film) filters are used. The interest in this last topic surely extends to fields other than astronomy.

An indication of the newness that has come into optics, changing it so radically from a formulism of classical physics, is given by D. Gabor of Imperial College, London, when he states that "optics was always considered as a good didactical preparation for wave mechanics; now it appears that quantum mechanics is not a bad preparation for optics" (page 30). Although it would appear that leadership in the "new optics" has come from Great Britain, France, Holland, Germany, and Italy, the contributions of the Americans Claude Shannon, Norbert Wiener, and Otto Schade are often mentioned by our colleagues overseas. In October 1951 a symposium on optical image evaluation was held at the National Bureau of Standards in Washington, attended by participants from many countries, and in June 1955 a symposium on the formation and evaluation of images was held at the University of Rochester. Several Americans attended the international conference in September 1954 in Florence, Italy, on "Problems in Contemporary Optics," which was, in a scientific sense, the forerunner of the Manchester symposium.

This volume is highly recommended to those who wish to become more familiar with the extent of modern optics and especially to astronomers and other optical folk who are interested in extracting the maximum amount of information to be obtained from the diffraction pattern that is called an optical image.

STANLEY S. BALLARD
Scripps Institution of Oceanography, University of California

Social Characteristics of Urban and Rural Communities, 1950. A volume in the *Census Monograph Series*. Otis D. Duncan and Albert J. Reiss, Jr. Wiley, New York; Chapman & Hall, London, 1956. 421 pp. $6.50.

The volume is an amplification and illumination of materials from the 1950 census dealing with the social characteristics of different-sized communities. The authors consider 11 classes of places, ranging from urbanized areas of three million or more inhabitants to sparsely settled farm regions. They set forth some interesting characteristics of these various bands in the sociological spectrum. Women outnumber men in cities and other incorporated places, but in the extremely rural regions the male animal predominates. The urban population in general is characterized by a higher median age, a lower fertility ratio, smaller families, higher percentages of separated and divorced persons, a larger percentage of women in the labor force, more years of education, higher incomes, and so on. These urban-rural differences might be easily surmised or discovered from other sociological writings, but their extent is here definitely stated and graphically illustrated. The authors have made an instructive and commendable contribution to social science.

BENJAMIN H. WILLIAMS
Industrial College of the Armed Forces

The Human Brain. From Primitive to Modern. A. M. Lassek. Thomas, Springfield, Ill., 1957. viii + 242 pp. $4.75.

The purpose of this book, the author says, "has been to try to portray the significance and impact of the long, past environment upon that dynamic organ, the human brain, and what it may mean to us in the middle of the 20th Century." The description of the brain itself and of the patterns of its working is brief

and schematic. The embryological and evolutionary development of the brain and its definitive structure as of today are given less than a sixth of the text, and this perhaps is adequate for Lassek's purpose. The rest of the book is devoted mainly to a survey of human cultures, drawn from the literature of cultural anthropology. The history of the evolution of the human mind is divided into four overlapping stages, represented by presavage, savage, barbarian, and civilized man.

The book is simply and clearly written and well indexed. There are a few errors and some infelicities of style. On page 13 we read that "the human race can be traced back with some degree of surety, only to about 5,000 B.C." I do not find the word "archtype" (pages 46, 51) in my dictionary. The frequent use of the word "data" as a singular noun in current literature is no justification for this barbarism. If "this data' (page 32) and "the data is" (page 67) may be regarded as good English, must we not also grant that "this datum are" would be equally acceptable?

For several reasons it is difficult to explain in vernacular language either the technical details or the general principles of cerebral structure and function. Lassek does not attempt to do this, but has chosen a different way to quicken popular interest in brains as the organs of civilization and in how they got that way. This he does by describing the ways of life in successive stages of cultural evolution.

C. JUDSON HERRICK

Grand Rapids, Michigan

Books Reviewed in The Scientific Monthly, December

Discovery of the Universe, G. de Vancouleurs (Macmillan). Reviewed by S. L. Lippincott.

The Next Hundred Years, H. Brown, J. Bonner, J. Weir (Viking). Reviewed by P. M. Stern.

The Life of Arthur Stanley Eddington, A. V. Douglas (Nelson). Reviewed by M. H. Wrubel.

The Direction of Research Establishments (H. M. Stationery Office).

Margarine and Other Food Fats, M. K. Schwitzer (Interscience). Reviewed by L. Voris.

Health and Medical Care in New York City (Harvard University Press). Reviewed by H. N. Pratt.

Rocks and Minerals, H. S. Zim and P. R. Shaffer (Simon and Schuster).

Reptiles, A. d'A. Bellairs (Hutchinson's University Library). Reviewed by R. F. Inger.

Annual Epidemiological and Vital Statistics, 1954 (World Health Organization).

Lascaux and Carnac, G. Daniel (Macmillan). Reviewed by F. de Laguna.

Plant Classification, L. Benson (Heath). Reviewed by K. L. Chambers.

A Textbook of Plant Virus Diseases, K. M. Smith (Little, Brown). Reviewed by W. C. Boyd.

Modern Science and the Nature of Life, W. S. Beck (Harcourt, Brace). Reviewed by M. Bates.

New Books

Organic Syntheses. An annual publication of satisfactory methods for the preparation of organic chemicals. vol. 37. James Cason, Ed. Wiley, New York; Chapman & Hall, London, 1957. 109 pp. $4.

Psychological Disorder and Crime. W. Lindesay Neustatter. Philosophical Library, New York, 1957. 248 pp. $6.

Calcium Metabolism. J. T. Irving. Methuen, London; Wiley, New York, 1957. 177 pp. $2.75.

Cities and Society. The revised reader in urban sociology. Paul K. Hatt and Albert J. Reiss, Jr. Free Press, Glencoe, Ill., 1957. 860 pp. $7.50.

The Social System of the High School. A study in the sociology of adolescence. C. Wayne Gordon. Free Press, Glencoe, Ill., 1957. 195 pp. $4.

Human Motivation. Probability and meaning. Fred T. Schreier. Free Press, Glencoe, Ill., 1957. 277 pp.

The Osteodontokeratic Culture of Australopithecus Prometheus. Memoir No. 10. Raymond A. Dart. Transvaal Museum, Pretoria, 1957. 113 pp.

Essentials of Human Anatomy. Russell T. Woodburne. Oxford University Press, New York, 1957. 628 pp. $12.50.

Nonparametric and Shortcut Statistics in Social, Biological, and Medical Statistics. Merle W. Tate and Richard C. Clelland. Interstate Printers and Publishers, Danville, Ill., 1957. 180 pp.

Laboratory Workbook for Principles of Zoology. John A. Moore, Oxford University Press, New York, 1957. 122 pp. $2.75.

Introduction to General Embryology. A. M. Dalcq. Translated by Jean Medawar. Oxford University Press, London, 1957. 184 pp.

Cahiers de Synthèse Organique. vol. II. Methodes et tableaux d'application. Jean Mathieu and André Allais. Masson, Paris, 1957. 322 pp.

Exploring Earth and Space. The story of the I.G.Y. Margaret O. Hyde. Whittlesey House, McGraw-Hill, New York, 1957. 160 pp. $3.

Integrating the Approaches to Mental Disease. Two conferences held under the auspices of the Committee on Public Health of the New York Academy of Medicine. H. D. Kruse, Ed. Hoeber-Harper, New York, 1957. 408 pp. $10.

Practical Astronomy. A new approach to an old science. W. Schroeder. Philosophical Library, New York, 1957. 217 pp. $6.

Quantum Mechanics. F. Mandl. Academic Press, New York; Butterworths, London, ed. 2, 1957. 277 pp. $6.50.

The Pacific Lowlands of Colombia. A Negroid area of the American tropics. Robert C. West. Louisiana State University Press, Baton Rouge, 1957. 292 pp. $5.

Poison on the Land. The war on wild life, and some remedies. J. Wentworth Day. Philosophical Library, New York, 1957. 256 pp. $6.

Parapsychology. Frontier science of the mind. A survey of the field, the methods, and the facts of ESP and PK research. J. B. Rhine and J. G. Pratt. Thomas, Springfield, Ill., 1957. 229 pp. $4.75.

High Energy Nuclear Physics. Proceedings of the seventh annual Rochester Conference, 15–19 April 1957. Compiled and edited by G. Ascoli, G. Feldman, L. J. Koester, Jr., R. Newton, W. Resenfeld, M. Ross, R. G. Sachs. Distributed by Interscience, New York, 1957. 491 pp. Paper, $4.50.

Contributions to the Theory of Games. vol. III. M. Resher, A. W. Tucker, P. Wolfe. Princeton University Press, Princeton, N.J., 1957. 441 pp. $5.

World Directory of Medical Schools. World Health Organization, Geneva, ed. 2, 1957 (order from Columbia University Press, New York). 314 pp. $5.

Higher Oxo Alcohols. Lewis F. Hatch. Wiley, New York, 1957. 130 pp. $4.50.

Body Water in Man. The acquisition and maintenance of the body fluids. Maurice B. Strauss. Little, Brown, Boston, 1957. 305 pp. $7.

The Hangover. A critical study in the psychodynamics of alcoholism. Benjamin Karpman. Thomas, Springfield, Ill., 1957. 554 pp. $9.50.

Methods of Biochemical Analysis. vol. 5. David Glick, Ed. Interscience, New York, 1957. 513 pp. $9.50.

A Concise Guide to Plastics. Herbert R. Simonds. Reinhold, New York; Chapman & Hall, London, 1957. 329 pp. $6.95.

The Patient and the Mental Hospital. Milton Greenblatt, Daniel J. Levinson, Richard H. Williams, Eds. Free Press, Glencoe, Ill., 1957. 676 pp. $6.

Atom Harvest. A British view of atomic energy. Leonard Bertin. Freeman, San Francisco, 1957. 253 pp. $3.25.

How to Know Freshwater Fishes. Pictured keys for identifying all of the freshwater fishes of the United States and also including a number of marine species which often enter freshwater. Samuel Eddy. Brown, Dubuque, Iowa, 1957. 259 pp. Cloth, $3.25; spiral bound paper, $2.75.

The Friendly Fungi. A new approach to the eelworm problem. C. L. Duddington. Faber and Faber, London, 1957 (order from Macmillan, New York). 188 pp. $4.50.

Dangerous Properties of Industrial Materials. A completely revised and enlarged edition of *Handbook of Dangerous Materials.* N. Irving Sax. Reinhold, New York; Chapman & Hall, London, ed. 2, 1957. 1472 pp. $19.50 (beginning Jan. 1958, $22.50).

The Economics of Communist Eastern Europe. Nicholas Spulber. Technology Press of Massachusetts Institute of Technology and Wiley, New York; Chapman & Hall, London, 1957. xxviii + 553 pp. $12.50.

Meetings and Societies

Amendment to AAAS Constitution

At the 1957 meeting of the AAAS Council, the Board of Directors will recommend changing the constitution of the Association to include past presidents as members of the Council. The specific change will be an amendment to Article IV, Section 2, which defines the membership and voting privileges of the Council, and will consist of insertion of the words *all past presidents* in the list of officers who serve as members of the Council.

This change is proposed as a means of putting into effect the wishes expressed by the Council last December in instructing the Board to "consider ways by which the interest of the past presidents of the American Association for the Advancement of Science may be maintained and their wisdom and experience utilized to the benefit of the Association."

This announcement is published to accord with the provisions for amending the constitution, which require publication at least one month prior to the December meeting of the Council.

Programs Planned for the AAAS Indianapolis Meeting

Section and society programs in psychology, the social and economic sciences, history and philosophy of science, industrial science, and science in general, to be presented at the Indianapolis meeting, are given here.

Programs in mathematics, physics, chemistry, astronomy, the earth sciences, biology, engineering, medicine, dentistry, pharmacy, and agriculture have been previously announced.

Psychology

Section I. Symposium: "Human Engineering: Research Planning for Space Flight"; arranged by Walter F. Grether, Wright Air Development Command, who will preside; 27 Dec., morning. "Zero and sub-gravity," John W. Senders, Minneapolis-Honeywell Regulator Company; "Sensory motor performance," George E. Long, Douglas Aircraft Company; "Environmental stress," John Lyman, University of California; "Social psychology aspects," Donald N. Michael, Dunlap & Associates, Inc.

Vice-presidential address, "Experiments in fear and conflict," by Neal E. Miller, Yale University; 27 Dec., afternoon; Clifford T. Morgan, Johns Hopkins University, presiding.

Symposium: "Psychopharmacology"; arranged by Sherman Ross, University of Maryland, who will preside; 28 Dec., morning. "Drugs and brain functioning," James Olds, University of Michigan; "Drugs and the biochemistry of behavior," Roger W. Russell, American Psychological Association; "Drugs and human perceptual and cognitive functioning," James G. Miller, University of Michigan; "Drugs and the problem of behavioral assessment," Sherman Ross; Discussion, Jonathan O. Cole, National Institute of Mental Health.

Symposium: "Effects of Early Experience on Behavior"; arranged by Austin Riesen, University of Chicago, who will preside; 28 Dec., afternoon. "The significance of environment in prenatal and early postnatal life," William R. Thompson, Wesleyan University; "On the generality of the effects of early experience," Howard F. Hunt, University of Chicago; "Has early visual environment a role in the development of visual discrimination in rats?" Eleanor J. Gibson and Richard D. Walk, Cornell University; Discussion, John Paul Scott, Roscoe B. Jackson Memorial Laboratory, and Seymour Levine, Ohio State University Medical Center.

Symposium: "Contemporary Research on Psycholinguistics"; arranged by Charles E. Osgood, University of Illinois, who will preside; 28 Dec., afternoon. "The perception of speech," Elvin Liverman, University of Connecticut; "Linguistics and the language of children," Roger Brown, Massachusetts Institute of Technology; "Word association as a tool in the analysis of language processes," James J. Jenkins, University of Minnesota; "Comparative psycholinguistics: the cross-linguistic approach," John B. Carroll, Harvard University.

Symposium: "Decision Theory, Signal Detection, and Psychophysics"; arranged by James P. Egan, Indiana University; 29 Dec., morning; W. P. Tanner, University of Michigan, presiding. "A decision-making theory of signal detection," John A. Swets, Massachusetts Institute of Technology; "Some tests and applications of the decision-theory model," J. C. R. Licklider, Bolt-Beranek and Newman, Inc.; "Operating characteristics in speech communication," James P. Egan.

Symposium: "Problems and Progress in Statistical Learning Theory"; arranged by Cletus J. Burke, Indiana University, who will preside; 29 Dec., afternoon. "Mathematical approaches to psychological problems," Cletus J. Burke; "The role of unobserved responses in two-choice situations: theory and experiment," David L. Laberge, Indiana University; "Effects of learning on perceptual response," Arnold M. Biner, Indiana University; "Models of behavior based on probability," Harry M. B. Hurwitz, University of London.

Social and Economic Sciences

Section K. Symposium, jointly with the National Academy of Economics and Political Science and the American Economic Association, with the collaboration of the National Social Science Honor Society Pi Gamma Mu: "Social Aspects of Urban Agglomeration"; arranged by Donald P. Ray, George Washington University; 27 Dec., evening; Carroll L. Christenson, Indiana University, presiding. "Metropolitan expansion and public administration," Luther H. Gulick, Institute of Public Administration; "Economic implications of urban growth," Coleman Woodbury, University of Wisconsin.

Vice-presidential address, "Statistical programming for problems of urban agglomeration," by Stuart A. Rice, Stuart Rice Associates, 27 Dec., evening.

Contributed papers; 30 Dec., afternoon; Donald P. Ray, George Washington University, presiding.

AAAS Committee on Social Aspects of Science. Symposium: "Social Aspects of Science as Illustrated by the Radiation Problem"; 29 Dec., afternoon; Chauncey D. Leake, Ohio State University, presiding. "General introduction to scientific problems with implications for society," Ward Pigman, University of Alabama Medical Center; Comments by members of the committee; "Social aspects of science as illustrated by the radiation problem," Barry Commoner, Washington University; "Technical factors in the radiation problem relating to social aspects of science," Marshall Brucer, Oak Ridge Institute of Nuclear Studies; "Biological factors in the radiation problem as illustrating social aspects of science," Charles L. Dunham, U.S. Atomic Energy Commission; "Medical factors in the radiation problem as related to social aspects of science," William G. Myers, Ohio State University;

Panel discussion by symposium participants and by members of the AAAS Committee on Social Aspects of Science.

American Political Science Association. Symposium, jointly with the Midwest Conference of Political Scientists and Section K: "Studies in Electoral Behavior"; arranged by Charles S. Hyneman, Indiana University, who will preside; 29 Dec., morning. "Contesting for office and voter participation: the case of Indiana," James A. Robinson, American Political Science Association; "Socio-economic factors and response to political issues in the 1956 presidential election," Warren E. Miller, University of Michigan; Discussion, Jean Driscoll, University of Wisconsin, Frank Munger, Syracuse University, and Philip S. Wilder, Wabash College.

American Sociological Society. Symposium, jointly with Section K: "Current Research on Population"; arranged by Vincent H. Whitney, Brown University, who will preside; 28 Dec., morning. "Demography as a science," Philip M. Hauser and Otis D. Duncan, University of Chicago; "The reproduction rate in Latin America," T. Lynn Smith, University of Florida; "Sterility, contraception, and expected size of white families," Pascal K. Whelpton and Arthur A. Campbell, Miami University, and Ronald Freedman, University of Michigan.

American Statistical Association. Address, cosponsored by Section K: "A statistically designed highway experiment," by P. E. Irick, AASHO Road Test, Ottawa, Illinois; 28 Dec., morning; H. W. Norton, University of Illinois, presiding. Discussion, C. F. Kossack, Purdue University.

Address, cosponsored by Section K: "Application of a mathematical model in plastic tooling research," by C. R. Hicks, Purdue University; 28 Dec., afternoon; D. L. Cheak, U.S. Naval Ordnance, presiding. Discussion, E. P. King, Eli Lilly and Company.

Society for the Advancement of Criminology. All SAC symposia are being held jointly with the Association for the Psychiatric Treatment of Offenders and the Institute for Research on Crime and Delinquency and are cosponsored by Section K. Symposium: "Advances in Theoretical Criminology and Penology"; 27 Dec., morning; Robert H. Gault, Northwestern University, presiding. "Interpreting the theory of individualism," Robert H. Gault; "Moral attitudes of American youth," H. H. Remmers, Purdue University; "Medicolegal approach to narcotic addiction," Morris Ploscowe, Joint American Medical Association and American Bar Association Committee on Drug Addiction; "Peripheral concepts in corrections," Negley K. Teeters, Temple University; "Sex crimes and sex perversions," James Reinhardt, University of Nebraska; "The new penology: fact or fiction," Alfred C. Schnur, Michigan State University.

Luncheon and address: "Wire tapping—an objective appraisal," by Donal E. J. MacNamara, New York Institute of Criminology; 27 Dec., noon; Morris Ploscowe presiding.

Symposium: "Advances in Interdisciplinary Approaches to Crime and Delinquency"; 27 Dec., afternoon; Herbert Bloch, Brooklyn College, presiding. "A critical analysis of the Royal Commission report on prostitution and homosexuality," Canio L. Zarrilli, New York Institute of Criminology; "Identifying and treating potential alcoholics," Roger J. Williams, University of Texas; "Analysis of prison disciplinary problems," Vernon Fox, Florida State University; "Selecting and training correctional personnel," Clyde Vedder, University of Arizona; "Problems of sentencing: practices and methods in American criminal courts," Morris Ploscowe; "The treatment of patients with behavioral reactions in a private mental hospital," Joseph Satten, Menninger Clinic.

Symposium on "Police-Crime"; 27 Dec., evening; Nicholas Pansegrouw, Institute for Research on Crime and Delinquency, presiding. Film on automobile fires, NATB; Discussion.

Symposium: "Advances in Police Administration"; 28 Dec., morning; Donal E. J. MacNamara presiding. "Police-fire integration in American cities," Charles James, Public Administration Service; "Metropolitan police districts: pro and con," Joseph Lohmann, sheriff, Cook County, Illinois; "The role of the policewoman in general police work," Lois Higgins, Illinois Crime Prevention Bureau; "New Jersey municipal police survey," Donal E. J. MacNamara; "Modern methods in police training," Roland Soule, Southern Police Institute; "An experiment in police personnel selection," John Duffy, New Jersey State Police Laboratory.

Luncheon and address: "Juvenile delinquency: myth or threat," by Herbert Bloch, Brooklyn College; 28 Dec., noon; Marcel Frym, Character Underwriters, Inc., presiding.

Symposium: "Advances in Scientific Crime Detection"; 28 Dec., afternoon; Robert Borkenstein, Indiana State Crime Laboratory, presiding. "Extracting flammable substances in arson investigations," Joseph Nicol, Michigan State University; "Problems in proving drunk driving," Robert Borkenstein; "A critique of field-laboratory relations in criminal investigations," John Duffy; "Problems in tool mark examinations," Roger S. Greene, California State Bureau of Criminal Identification and Investigation; "Scientific crime detection versus privacy and civil rights," Donal E. J. MacNamara; "Criminalistics: the thin line of success," Paul L. Kirk, University of California.

History and Philosophy of Science

Section L. Symposium, cosponsored by Section Np and the Philosophy of Science Association: "Can Science Provide an Ethical Code?"; arranged by Lewis K. Zerby, Michigan State University; 29 Dec., morning; Hermann J. Muller, Indiana University, presiding. "Physics," Henry Margenau, Yale University; "Biology and history of science," Chauncey D. Leake, Ohio State University; "Social science and the philosophy of science," Richard Rudner, Michigan State University.

Contributed papers, cosponsored by Section Np and the Philosophy of Science Association; 29 Dec., afternoon; Norwood Russell Hanson, Indiana University, presiding.

Contributed papers, cosponsored by the Philosophy of Science Association; 30 Dec., morning; C. Doris Hellman, Pratt Institute, presiding.

Vice-presidential address, "The history of science and the problem of understanding the science of today," by I. Bernard Cohen, Harvard University, 30 Dec., morning.

Society for General Systems Research. Symposium: "Organization for Humans, Cells, and Artifacts"; arranged by Richard L. Meier, University of Michigan, who will preside; 27 Dec., morning. "Attempts to generalize theorems on internal entropy production," Caxton Foster and Anatol Rapoport, University of Michigan; Presidential address, "Some political implications of general systems," Kenneth E. Boulding, University of Michigan; "Organized systems with discrete information transfer, Manfred Kochen, I.B.M. Research Center; Discussion.

Industrial Science

Section P. Second Industrial Science Award, 26 Dec., evening; Allen T. Bonnell, Drexel Institute of Technology, presiding.

Symposium, cosponsored by Section K: "Science, Technology, and General Welfare in a Capitalistic Society"; arranged by Allen T. Bonnell; 27 Dec., morning; Frank C. Croxton, Battelle Memorial Institute, presiding. "Welfare implications of accelerated technological change," Louis C. McCabe, Resources Research, Inc.; "The reconciliation of company profits and public welfare," G. E. Kimball, Arthur D. Little, Inc.; "Pollution control—some of industry's problems and opportunities," Robert Doolittle, Youngstown Sheet and Tube Company.

Vice-presidential address, "The companies we keep," by Monroe E. Spaght,

Shell Oil Company, 27 Dec., noon; Frank C. Croxton, presiding.

Society for Industrial Microbiology, Washington Section. Symposium, cosponsored by Section P: "Some Areas in Industrial Microbiology"; 28 Dec., afternoon; W. D. Stewart, Atlantic Research Corporation, presiding. Introduction, W. D. Stewart; "Microbiology in the fermentation industries," J. E. McClary, Anheuser-Busch, Inc.; "Microbiologists in the prevention of deterioration," Carl J. Wessel, National Academy of Sciences; "Industrial microbiology in the pharmaceutical industry," J. M. McGuire, Lilly Research Laboratories; "Microbial research in the Bureau of Mines," Walter N. Ezekiel, U.S. Bureau of Mines.

Science in General

Conference on Scientific Editorial Problems. Opening session, cosponsored by the Society of Technical Writers and Editors: "Correlation and Dissemination of Scientific Data"; arranged by William C. Miller, Magnavox Company; 26 Dec., afternoon; Jacques Harlow, Magnavox Company, presiding. Address by the new national president of the Society of Technical Writers and Editors; "Vehicles required for communication in a scientific organization," R. G. Chollar, National Cash Register Company; "Importance of technical report preparation as a communications medium," Paul Robinson, General Electric Company.

"Scientific Translations"; moderated by J. George Adashko, College of the City of New York; 27 Dec., morning. "Research from foreign language periodicals and documents," Ralph E. O'Dette, National Science Foundation; "NIH translation program," Scott Adams, National Institutes of Health; "Making translations available," Herman Henkle, John Crerar Library; "Translating one million words a month," Earl Coleman, Consultants Bureau, New York.

"The Indispensability of the Editorial Function in the Communication of Scientific Information"; 28 Dec., morning; Harry F. Arader, University of Pennsylvania, presiding. "Developing editorial objectivity in the scientist," Joseph D. Chaplin, Philco Corporation; "Communication theory in scientific editing," Andrew W. Bluemle, University of Pennsylvania; "The universality of editorial principles," Christian K. Arnold, Pennsylvania State University.

"Meeting the Challenge in Technical Writing"; cosponsored by the Society of Technical Writers and Editors; arranged by William C. Miller; 28 Dec., afternoon; M. Russell Hill, Campbell-Ewald Company, moderator. "Responsibilities of technical writers for the needs of technical readers," W. Earl Britton, University of Michigan; "Graphic arts in technical writing," Wayne F. Feldman, Recording and Statistical Corporation; "Relationship between the technical library and technical writing," Herman Henkle.

Conference on Scientific Manpower. Two-session symposium, cosponsored by the Engineering Manpower Commission, the Scientific Manpower Commission, the National Research Council, the National Science Foundation, and Section M: "Scientists and Scientific Research in a Changing Economy"; 30 Dec., morning and afternoon. Part I, Robert C. Turner, Indiana University, presiding: "Scientific advance as a factor in economic change," Yale Brozen, Northwestern University; "New and changing activities of scientists," Julian W. Feiss, Kennecott Copper Company; "The support of scientific research," William D. Carey, U.S. Bureau of the Budget; Discussion. Part II, Ralph E. Cleland, Indiana University, presiding: "New dimensions in scientific training," Herbert E. Longenecker, University of Illinois; "Research centers—importance in scientific research and potential for training," Ralph D. Bennett, General Electric Company; "Comparative salary and income level of scientists," Thomas J. Mills, National Science Foundation; Discussion.

International Ornithological Congress

The National Science Foundation will award grants to defray partial travel expenses for a limited number of American scientists who wish to participate in the 12th International Ornithological Congress, which is scheduled to meet in Helsinki, Finland, 5–12 June 1958. Application blanks, returnable before *20 January 1958,* may be obtained from the National Science Foundation, Washington 25, D.C.

Biophysical Society

The Biophysical Society will hold its 1958 national meeting 5–7 February in Cambridge, Mass. There will be special emphasis this year on "Microsomal particles and their structure and function" and on "Muscle proteins and contractile mechanisms." Several plenary sessions will be devoted to these subjects, and a majority of the specially invited speakers will contribute to these programs. There will, however, be additional special programs at which invited speakers will review progress in other fields of biophysics. Financial support by the National Science Foundation has made it possible to include among the invited speakers several outstanding scientists from abroad as well as from this country.

About half of the total meeting time will be devoted to short contributed papers in parallel sessions covering a wide range of special areas of biophysics, and for these programs, guests as well as members may offer abstracts. Abstracts of not over 250 words should be sent by *2 December* to the secretary, Samuel A. Talbot, Johns Hopkins Hospital, Baltimore, Md., who will supply further information on request.

Forthcoming Events

December

26–30. **American Assoc. for the Advancement of Science, annual, Indianapolis, Ind. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5.)**

27. Association for Symbolic Logic, Cambridge, Mass. (J. Barlaz, Rutgers Univ., New Brunswick, N.J.)

27–28. Linguistic Soc. of America, Chicago, Ill. (A. A. Hill, Box 7790, University Station, Austin 12, Tex.)

27–30. American Finance Assoc., annual, Philadelphia, Pa. (G. E. Hassett, Jr., New York Univ., 90 Trinity Pl., New York 6.)

28–29. American Folklore Soc., annual, Chicago, Ill. (M. Leach, Box 5, Bennett Hall, Univ. of Pennsylvania, Philadelphia 4, Pa.)

28–30. American Anthropological Assoc., annual, Chicago, Ill. (W. S. Godfrey, Jr., Logan Museum, Beloit College, Beloit, Wis.)

28–30. American Economic Assoc., annual, Philadelphia, Pa. (J. W. Bell, Northwestern Univ., Evanston, Ill.)

28–30. Archaeological Inst. of America, annual, Washington, D.C. (C. Boulter, 608, Univ. of Cincinnati Library, Cincinnati 21, Ohio.)

28–30. Econometric Soc., Philadelphia, Pa. (R. Ruggles, Dept. of Economics, Yale Univ., New Haven, Conn.)

28–30. History of Science Soc., annual, New York, N.Y. (Miss M. Boas, Brandeis Univ., Waltham 54, Mass.)

January

6–8. Reliability and Quality Control, 4th natl. symp., Washington, D.C. (C. M. Ryerson, RCA Bldg. 10–6, Camden 2, N.J.)

7–10. Radioactive Isotopes in Clinical Application and Research) 3rd internatl. symp., Bad Gastein, Austria. (Second Medical Clinic, Vienna Univ., Vienna, Austria.)

8–10. Northeastern Weed Control Conf., 12th annual, New York. (R. J. Aldrich, Farm Crops Dept., Rutgers Univ., New Brunswick, N.J.)

13–17. Society of Automotive Engineers, annual, Detroit, Mich. (Meetings Div., SAE, 29 W. 39 St., New York 18.)

17–18. Blood Symposium, 7th annual, Detroit, Mich. (W. H. Seegers, Dept. of Physiology and Pharmacology, Wayne State Univ. College of Medicine, 1401 Rivard, Detroit 7.)

22–24. American Council of Learned

Societies, 39th annual, Bloomington, Ind. (ACLS, 2101 R St., NW, Washington 8.)

22–25. American Group Psychotherapy Assoc., 15th annual, New York. (M. Berger, 50 E. 72 St., New York 21.)

27–28. Scintillation Counter Symp., Washington, D.C. (G. A. Morton, Radio Corporation of America, Princeton, N.J.)

27–29. American Soc. of Heating and Air-Conditioning Engineers, Pittsburgh, Pa. (A. V. Hutchinson, ASHAE, 62 Worth St., New York 13.)

27–30. American Meteorological Soc., 163rd natl., New York. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

27–31. Institute of Aeronautical Sciences, 26th annual, New York, N.Y. (S. P. Johnston, IAS, 2 E. 64 St., New York 21.)

28–30. Aging, 4th Ciba Foundation Colloquium (by invitation), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

28–30. American Mathematical Soc., 64th annual, Cincinnati, Ohio. (J. H. Curtiss, AMS, 190 Hope St., Providence 6, R.I.)

29–1. American Physical Soc., annual, New York, N.Y. (K. K. Darrow, Columbia Univ., New York 27.)

30–31. College–Industry Conf., American Soc. for Engineering Education, 10th annual, Ann Arbor, Mich. (W. D. McIlvaine, College of Engineering, Ann Arbor.)

30–1. American Assoc. of Physics Teachers, New York. (F. Verbrugge, Univ. of Minnesota, Minneapolis.)

30–1. Western Soc. for Clinical Research, 11th annual, Carmel-by-the-Sea, Calif. (A. J. Seaman, Univ. of Oregon Medical School, Portland 1.)

31. Mathematical Assoc. of America, annual, Cincinnati, Ohio. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

31–1. Problems of Geriatrics, symp. (by invitation only), New York. (B. F. Chow, Johns Hopkins Univ., School of Hygiene and Public Health, 615 N. Wolfe St., Baltimore 5, Md.)

February

1–14. Pan American Assoc. of Ophthalmology, Caribbean cruise cong., sailing from New York, N.Y. (L. V. Arnold, 33 Washington Sq. W., New York 11.)

3–4. Progress and Trends in Chemical and Petroleum Instrumentation, Wilmington, Del. (H. S. Kindler, Instrument Soc. of America, 313 Sixth Ave., Pittsburgh 22, Pa.)

3–7. American Inst. of Electrical Engineers, winter genl., New York, N.Y. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

10–14. American Soc. for Testing Materials, St. Louis, Mo. (F. F. Van Atta, ASTM, 1916 Race St., Philadelphia 3, Pa.)

16–20. American Inst. of Mining, Metallurgical and Petroleum Engineers, annual, New York. (E. O. Kirkendall, AIME, 29 W. 39 St., New York 18.)

20–21. Transistor and Solid State Circuits Conf., Philadelphia, Pa. (J. H. Milligan, Jr., Dept. of Electrical Engr., New York Univ., New York 53.)

24–28. American Soc. of Civil Engineers, Chicago, Ill. (W. W. Wisely, ASCE, 33 W. 39 St., New York 18.)

March

1. Junior Solar Symposium, Tempe, Ariz. (Association for Applied Solar Energy, 3424 N. Central Ave., Phoenix, Ariz.)

5–6. Gas Conditioning Conf., 7th annual, Norman, Okla. (M. L. Powers, Extension Div., Univ. of Oklahoma, Norman.)

6–8. Fundamental Cancer Research, 12th annual, Houston, Tex. (W. K. Sinclair, M. D. Anderson Hospital and Tumor Inst., Univ. of Texas, Houston 25.)

6–8. Optical Soc. of America, annual, New York. (A. C. Hardy, Massachusetts Inst. of Technology, Cambridge 39.)

10–13. American Assoc. of Petroleum Geologists, annual, Los Angeles, Calif. (R. H. Dott, AAPG, Box 979, Tulsa 1, Okla.)

16–21. Nuclear Engineering and Science Cong., Chicago, Ill. (D. I. Cooper, *Nucleonics*, 330 W. 42 St., New York.)

17–21. National Assoc. of Corrosion Engineers, 14th annual, San Francisco, Calif. (NACE, Southern Standard Bldg., Houston 2, Tex.)

18–20. Amino Acids and Peptides, Ciba Foundation symp. (by invitation), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

EQUIPMENT NEWS

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Science *does not assume responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to* Science, *Room 740, 11 W. 42 St., New York 36, N.Y. Include the name(s) of the manufacturer(s) and the department number(s).*

■ ALTITUDE TEST CHAMBER includes three modes of application. A combination chamber simulates altitude to 200,000 ft with temperatures from −100° to +400°F. The altitude chamber may be lifted out and operated separately at ambient temperatures. The remaining apparatus operates independently to provide controlled-temperature environments. Sizes from 12 to 36 ft^3 are available. (Mantec, Inc., Dept. S746)

■ SINE-WAVE-TO-SQUARE-WAVE CONVERTER utilizes the signal from the driving oscillator to provide operating power for its transistor squaring circuit. The unit is capable of 17 v peak-to-peak output from 5 to 100,000 cy/sec. Rise time is 0.5 percent of period. (Mandrel Industries Inc., Dept. S747)

■ ANGULAR ACCELERATION GENERATOR is a torsional pendulum instrument incorporating a 12-in. test-bed table which provides very low-decrement sinusoidal oscillations to a maximum amplitude of ±180 deg. The period of oscillation is variable from 0.92 to 8 sec, and angular acceleration is variable between 0.48 and 146 $rad/sec.^2$ Test devices weighing up to 25 lb can be accommodated. Angular displacement may be derived from a micropotentiometer or synchro driven from the torsion shaft. (Statham Development Corp., Dept. S753)

■ PHASE NULL METER, small enough to be built into equipment, consists of a phase-sensitive vacuum-tube voltmeter and a calibrated phase shifter. Accuracy is ±1 deg over the ranges ±20 deg and 160 to 200 deg. A push-button polarity switch informs the operator whether the signal is closer to 0- or 180-deg phase relation to the reference. Frequency range is 380 to 420 cy/sec. Input signal range is 300 to 800 mv; reference voltages from 3.15 to 30 v, a-c, can be utilized. (Trio Laboratories, Inc., Dept. S756)

■ OXYGENATOR operates by exposing to an oxygen atmosphere a film of blood on a series of rotating disks of stainless steel mounted within a horizontal cylinder of Pyrex. No foaming or bubbling of the blood is said to occur. Oxygen-CO_2 mixtures are delivered the full length of the apparatus by a perforated stainless-steel

tube. Three lengths, 13, 17 and 21 in., with 60, 80 and 100 disks, respectively, are available. Corresponding priming volumes are 1400, 2000 and 2500 ml. (Pemco, Inc., Dept. S765)

■ THICKNESS GAGE uses radiation from a sealed, self-contained gamma-radiation source to measure thickness of continuous sheet materials. Transmitted radiation is sensed by a scintillation method. Response time is in the millisecond range. (Budd Co., Dept. S760)

■ FREEZE DRYER is mechanically refrigerated to temperatures as low as −60°C. The equipment, of mobile design, provides for bulk drying as well as manifold drying. Heat is furnished by quartz infrared tubes mounted inside the stainless-steel vacuum drum. The condenser has a capacity of 16 lit. A McLeod gage provides measurement of vacuum, and a thermistor indicates condenser temperature. (VirTis Company, Inc., Dept. S761)

■ VIBRATION EXCITER of electrodynamic design has a frequency range from 5 to 5000 cy/sec. Force levels of 1025 lb r.m.s. and 3150 lb peak are obtainable in noise testing with 15-to-2000-cy/sec bandwidth. The exciter will operate in altitude environments from 0 to 125,000 ft, in humidity from 0 to 95 percent, and in temperature from 0 to 200°F. Total displacement is 1 in. (MB Manufacturing Co., Dept. S763)

■ OSCILLOSCOPE has identical horizontal and vertical deflection characteristics for use as an *x-y* indicator. It may be converted into a general-purpose instrument by interchanging plugged-in components. Bandpass is from d-c to 10 Mcy/sec, rise time is 0.35 μsec, and deflection factors are selectable from 0.05 to 20 v per division. (Tektronix, Inc., Dept. S766)

■ FRACTIONATING COLUMN of spinning-band design features 2 to 3 ml liquid holdup, approximately 0.1 mm-Hg pressure drop per plate, 30 theoretical plates at total reflex, and 60 percent efficiency at a liquid take-off rate of 43 ml/hr. (Stanford Glassblowing Laboratories, Dept. S767)

■ PRESSURE CHAMBER, ½ in. in diameter by 10 in. high, provides pressures to 30,000 atm. The pressure chamber is inside a thick-walled tube which in turn is surrounded by a stack of restraining rings. A tapered fit between the tube and the rings permits application of squeeze to prevent radial expansion. Pumps are manually operated. Electrically insulated leads may be conducted into the chamber. (Nucor Research, Inc., Dept. S771)

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Microbiologist, Ph.D., virology, cellular metabolism, genetics; biochemical, biophysical techniques; medical teaching and research experience; seeking more active position; can do. Box 295, SCIENCE. 11/29

Phytopathologist; Ph.D., 37. Mycology, entomology, bacteriology, and chemistry background; experiment station and industrial experience in plant diseases, bioassay, and fungicides. Research position desired. Box 303, SCIENCE. X

POSITIONS WANTED

(a) **Biochemist**; M.S., biochemistry; 10 years, chemist, large industrial company; 4 years as director of biochemistry government research unit. (b) **Science Writer**; B.S.; 12 years' experience in all phases of writing, editing, and production of magazines and books; managed editorial department of 45 employees. Medical Bureau, Burneice Larson, Director, 900 North Michigan Avenue, Chicago. X

Zoologist–Physiologist, Ph.D. Desires teaching position with opportunity for research. Age, 37. Capable of assuming responsibilities at senior staff level. Now group leader in physiology at government institute. Broad background and interests. Box 301, SCIENCE. X

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(a) **Analytical Chemist** preferably with bioanalytical experience and, also, industrial experience. **Biochemist** or **Development Chemist** with master's of higher and experienced in isolation of natural compounds and **Chemical Engineer** experienced in fermentation or chemical pilot plant work; large industrial company; Pacific Coast. (b) **Clinical Physiologist** experienced in respiratory physiology; preferably physician, Ph.D. eligible; duties: research teaching at graduate and postgraduate levels; university medical school; new teaching hospital; East. (c) **Associate Medical Director**; preferably physician experienced in pharmaceutical field; major company, East. (d) **Copywriter**; science degree with experience in pharmaceutical copywriting; well-known manufacturer of pharmaceutical and biologicals. S-11-29 Medical Bureau, Burneice Larson, Director, 900 North Michigan Avenue, Chicago. X

Bacteriologist, Ph.D. Full-time research appointment (with academic privileges) at established laboratory of midwestern university dental school. Salary range: $5500–$6500, other benefits. Excellent facilities, opportunity to publish an original research on physiology of oral microorganisms. Give detailed qualifications in reply. Box 300, SCIENCE. 12/6

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(a) **Biochemist**; Ph.D. interested research ultramicro chemistry, development new diagnostic tests; head clinical department, 500-bed general hospital; $8400–$10,000; city 150,000, mideast. (b) **Chief Bacteriologist**; M.S., Ph.D. experienced clinical work; head department, all new 600-bed medical center hospital; South. (c) **Bacteriologist**; M.S., Ph.D. to head department, 300-bed general hospital; outstanding facilities, equipment, research possibilities; to $7500; Chicago area. (d) **Research Biochemist**; M.S., recent Ph.D. for research protein chemistry; important southern university medical school; possible faculty appointment later; to $6000. (e) **Biochemist**; M.S. to have full charge of department, hospital expanding to over 500 beds; college-affiliated technology school; to $7200; Mideast. Woodward Medical Bureau, Ann Woodward, Director, 185 N. Wabash Avenue, Chicago. X

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Electron Microscopist. An electron microscopist is needed at the biological laboratories of Harvard University, Cambridge 38, Mass., to collaborate in research with members of the department. Salary to depend upon experience and other qualifications. Inquiries should be addressed to the Chairman, Electron Microscope Committee, at the above address. 11/29; 12/6, 13

Foreign Language Indexers. Individuals with backgrounds in physiology or pharmacology and fluency in Italian, Spanish, Portuguese, and other foreign languages to index medical literature. Part-time work at home near good medical library. Box 304, SCIENCE. 12/6

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Free-Lance Writer. Experienced. Strong background in medical or ancillary sciences. Will need facilities of medical library. Must be able to write upon assignment and meet deadlines. Medical Arts Publishing Foundation. 1603 Oakdale Street, Houston 4, Texas. 11/22, 29; 12/6

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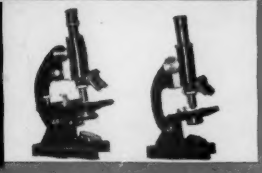

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